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Values in Action: Simplicity, Completeness, and Carefulness in the Development of the Systematisations of Chemical Elements

This dissertation is submitted for the degree of Doctor of Philosophy

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Declaration

This dissertation is the result of my own work and includes nothing which is the outcome of work done in collaboration except as declared in the Preface and specified in the text.

It is not substantially the same as any that I have submitted, or, is being concurrently submitted for a degree or diploma or other qualification at the University of Cambridge or any other University or similar institution except as declared in the Preface and specified in the text. I further state that no substantial part of my dissertation has already been submitted, or, is being concurrently submitted for any such degree, diploma or other qualification at the University of Cambridge or any other University or similar institution except as declared in the Preface and specified in the text.

This dissertation does not exceed the prescribed word limit for the relevant Degree Committee.

Abstract

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Karoliina Julia Pulkkinen

This thesis demonstrates how three chemists – Dmitrii Ivanovich Mendeleev, Julius Lothar Meyer, and John Newlands – emphasised different values when developing their systematisations of the chemical elements in 1863-1875. While no chemist emphasised just one value in the course of establishing of their systematisations, I argue that Newlands elevated simplicity ("simple relation"), Mendeleev completeness (*polnost'*), and Meyer carefulness when systematising the elements. This thesis sets to show that values provide an illuminating framework to articulate the differences among the competing periodic systems, and give us a novel reading of the priority dispute concerning the discovery of the periodic system.

By examining the role of values in the systematisation of the elements, this thesis seeks to give an example of an integrated history and philosophy of science (iHPS) approach to values. As integrating history and philosophy of science introduces some methodological challenges, I will start by arguing in favour the hermeneutic model to iHPS as articulated by Jutta Schickore (Ch.1). Chapters 2-5 are largely historical. Chapter 2 introduces historical background to the challenge of systematising the elements in the nineteenth century. I then demonstrate how Newlands (Ch.3), Meyer (Ch.4), and Mendeleev (Ch.5) emphasised different values in the course of developing their systematisations of the chemical elements.

Chapters 6-8 are more philosophical. In chapter 6, I argue in favour of specific abstract characterisations of simplicity, completeness, and carefulness on the basis of the historical material presented in chapters 3-5. After arguing in favour of a specific understanding of values, I identify a distinct relationship between the chemists' subsequent uses of the systems and their emphasis on specific values during the development of their systems. In particular, I argue that valuing of completeness contributed to Mendeleev's use of the system for making predictions (Ch.7), whereas valuing of carefulness did much to support using Meyer's system for error determination (Ch.8).

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Table of Contents

Prologue

Imagine not having heard of the lightness of hydrogen, the strength of titanium, or that cerium catches fire when it is scratched. If you knew only little about the chemical elements, the following statement from the Italian chemist and author Primo Levi would probably not resonate with you: “[Mendeleev’s periodic table was] loftier and more solemn than all the poetry we had swallowed down in liceo, and come to think of it, it even rhymed” (Levi 1985, 41). You might take Levi’s word for it because of his expertise in chemistry, but to grasp what Levi implied by his analogy requires some background knowledge. Our teachers might explain that though the similar groups are housed on the vertical columns of the system, Levi’s rhyme derives from its horizontal lines. They might point out how on the second period, we can find the least metallic members, or how on the third period, we encounter the elements most abundant on earth. However, the reason why our teachers could explain the table’s rhyme in this manner is that they have been taught to look for it by their teachers, who themselves learnt to listen out for it from their teachers. And, eventually, we reach a point in the chain of pedagogues where we get to those who were not taught its rhyme by their teachers but learned to listen out for it (mostly) on their own.

This thesis concerns how those teachers first heard its rhyme. More specifically, my aim here is to map how the chemists developed their periodic systems so as to help others to listen to it, too. Many associate the Russian chemist Dmitrii Ivanovich Mendeleev with the process of developing the periodic system, but Mendeleev was at the tail end of a group of five chemists who developed systematisations which are now recognised as the precursors of the modern periodic system. Before Mendeleev, Alexandre-Émile Béguyer de Chancourtois (1862), John Newlands (1864-6), William Odling (1864), Gustav Hinrichs (1869) and Julius Lothar Meyer (1864, 1868, 1870), created systematisations that historians and philosophers of science recognise as the precursors of the periodic system we have today (van Spronsen 1969; Scerri 2007; Gordin 2012).

In this thesis, I provide a new account of the processes of developing some of the early periodic systems. I do so with the help of the framework of values in science. In particular,

I utilise the values framework to show how three of the abovementioned chemists – Newlands, Meyer, and Mendeleev – developed their systematisations of the elements. I will argue that Newlands elevated a quality that he referred to as a "simple relation." In contrast, Meyer shunned the idea that the periodic systems expressed simple laws. Instead, Meyer stressed the importance of being selective on which observations could serve as a foundation for a periodic system, and he employed a number of interrelated strategies for signalling the quality of observations. To analyse the distinctness of Meyer's approach to systematisation, I introduce a new analytic category of carefulness, which has not been discussed in the existing philosophical literature on values in science. Mendeleev was much more permissive than Meyer on what kinds of observations to include in his system. For Mendeleev, it was more important that the system was as complete as possible. Although there were some other valued qualities involved in the process of developing the systems, I will argue that Newlands emphasised simplicity, Meyer carefulness, and Mendeleev completeness over other values in the course of developing their systematisations.

The helpfulness of values at detailing the development of the periodic systems arises from the flexibility and broadness of the category, which allows adapting the framework to different contexts. The broadness becomes apparent in historian Loren Graham's understanding of values as "what people think to be good." Graham highlights that this understanding "encompasses the broad range of normative judgments which all human beings make, and which can be further broken down to various types of sociopolitical values (e.g., moral, political, esthetic, religious)" (Graham 1981, 4; see also Kincaid, Dupré, and Wylie 2007,10).

Apart from values giving us a flexible category that encompasses a wide range of normative judgements, the framework is useful because values are recognised as factors that guide decision-making. In the words of historian Matthew Stanley, "[g]enerally, we can think of values as defining principles, desired objectives, and preferred methods. They shape decisions, particularly difficult ones. When a choice must be made, how does one decide? … Values are that which people are willing to hold to even in difficult circumstances" (Stanley 2007, 5). As we will see, the process of constructing a system of

elements involved many choices, including decisions on how to present atomic weights; which elements to include in the system; which qualitative similarities between the elements to prioritize, and which ones to downplay. Stanley's observation on how values "serve as moderators and stimuli of action and decisions" highlights that the values framework can be informative in tracking how the early periodic systems took their shape (Stanley 2007,5).

In this thesis, I follow Graham and Stanley in their broad understanding of values. I specifically understand values as the properties or qualities that chemists emphasised in the course of systematising the chemical elements. Furthermore, what makes the values framework helpful for an investigation that combines historical and philosophical approaches is that the framework is utilised both by historians and philosophers. As noted earlier, both Graham and Stanley are historians of science. Also the philosophical literature on values in science is vibrant. To illustrate, values feature in debates on inductive risk (Rudner 1953; Levi 1960; Douglas 2009; John 2015); theory choice and change in science (Kuhn 1977, 320-351; Laudan 1984; Schindler 2018); epistemic/nonepistemic distinction (McMullin 1982; Rooney 1992; Steel 2010; Douglas 2013; Rooney 2017); communal norms guiding science (Longino 1990; Longino 1997, 39-58); inference to the best explanation (Lipton 2004); ideals of science (Lacey 1999; Douglas 2009); and the role of social and political values in scientific practice (Douglas 2009; Brown 2013; Elliott 2017, Elliott and Steel 2017).[1]

The number and variety of investigations of values in science demonstrate the flexibility of the framework. However, so far, there has been a scarcity in highly detailed accounts that combine historical and philosophical approaches. Although especially Thomas Kuhn (1977, 320-351), Ernan McMullin (1982) and Phyllis Rooney (1992) emphasise the importance of taking the historical context into account when we discern the complex roles of values, their analyses on individual values are rather brief. In contrast, the value of simplicity has received more in-depth attention. An especially commendable example is Elliott Sober's account of Occam's razors and their role in justifying theories, where Sober

[1] For helpful overviews on the debate on values in science see especially Kincaid, Dupré, and Wylie (2007), Douglas (2016), Elliott (2017) and Elliott and Steel (2017).

evokes a rich array of historical resources for his more normative arguments about the role of simplicity in science (Sober 2015). Although this thesis does not follow Sober's lead in teasing out whether values contributed to the justification of the periodic systems, in a similar vein, I aim to provide a philosophical analysis of values which substantially draws from the historical context.

The prioritisation of the *development* of scientific representations over their justification (or reception) should not be seen as rendering this thesis less philosophical, although traditionally, the context of development has been viewed as belonging to the turf of historians rather than philosophers (Schickore and Steinle 2007; Anderson 2006; also Elliott and McKaughan 2009). As I will show, taking the context of development seriously will help to provide a more thorough abstract analysis of values. The distinctly philosophical goal of this thesis is to demonstrate how our philosophical analyses of individual values and our broader categories of values benefit from a closer engagement with the historical sources. Furthermore, I will argue that values emphasised in the processes of development supported the chemists' further uses of the periodic systems.

Ideally, this investigation would not limit itself just to the systems of the three chemists, but encompass all works that constitute the earliest recognitions of periodic systems. However, there are a number of practical reasons prohibiting me to extend the scope of this thesis to encompass the systems of Chancourtois, Odling, Hinrichs, and Baumhauer. One of the reasons is that among all the chemists, Newlands, Meyer and Mendeleev left the richest trail of different versions to compare and contrast. The abundance of different versions is helpful because we get a clear view of the decisions that went into constructing the systematisations. However, there is also a more mundane reason for the exclusion of the systems of other chemists. In the light of the high number of articles and accounts on the periodic system published by Newlands, Meyer, and Mendeleev, the number of chemists had to be kept low in the interest of providing a thorough account of their processes of developing their systematisations.

This thesis has the following structure. As my aim is to give an account of values that effectively integrates historical and philosophical approaches, I will start with a thorough

methodological reflection (Ch.1). In particular, I will argue in favour of Jutta Schickore's hermeneutic approach to integrated history and philosophy of science (iHPS) in analysing the role of values in science. Although I argue for giving greater attention to particular values (such as simplicity, carefulness, and completeness), it is important to note that philosophers have traditionally been more drawn to treating values as collective groups (Douglas 2013, 796). In chapter 1, I will argue that in-depth analyses of the particular values in their historical context can have great import for our collective or general accounts of values. In the second part of chapter 1 then, I suggest that it would be helpful to resist our tendency to organise values into general categories, and instead, start using the vocabulary of different *functions* of values.

After the methodological chapter, chapter 2 offers a brief overview of the task of systematising the chemical elements in the mid-nineteenth century. Following the chronological order in which the chemists published their systems, I will first introduce the systems of Newlands (Ch.3). After doing so, I proceed to Meyer's systems (Ch.4) and then outline Mendeleev's process of developing his periodic system (Ch.5). As the writings of Mendeleev and Meyer are more voluminous than those of Newlands, the balance of the length of my discussion reflects that discrepancy.

After the historical sections of this thesis, I argue in favour of specific abstract characterisations of simplicity, carefulness, and completeness (Ch. 6). Greater awareness of the specific values will then help to demonstrate a relationship between values emphasised in the course of developing the systems, and the chemists' further uses of their systematisations. In particular, I will argue that Mendeleev's valuing of completeness supported using the system for making predictions (Ch.7). In chapter 8, I will argue that Meyer's valuing of carefulness allowed Meyer to use the system as a diagnostic tool to identify errors in experimental results. Chapter 9 then concludes the discussion by highlighting how this thesis contributes to the philosophical (and historical) literature on periodic system, and the philosophical and historical accounts on values in science.

Chapter 1. The Hermeneutic Model and Values in Science

1.1 Introduction

Values have received considerable attention in philosophy of science in past decades, but so far, accounts of values in chemistry have been scarce. In this thesis, I investigate the role of values during the development of the early periodic systems of elements. By looking into the role of values in the construction of the periodic systems, this thesis combines historical and philosophical approaches to shed light on values in science. Such a combination calls for reflecting on *how* to bring together historical and philosophical approaches to values. For this reason, the main aim of this chapter is to explain how I have configured the relationship between history and philosophy of science in this particular integrated history and philosophy of science (iHPS) investigation. From the available methodological models of iHPS, I will argue that Jutta Schickore's hermeneutic model (2011) is especially suited for explicating the role of values in science.

The discussion of this chapter is structured as follows. In section 1.2, I introduce the hermeneutic model and argue that it is especially suited for explicating the role of values in science. In contrast to Schickore, however, I argue that applying the hermeneutic approach requires showing how the model has been followed in practice. While such transparency injects a level of autobiographical reflection that is unusual in philosophy, I argue that such transparency is needed for the full execution of the hermeneutic model. For this reason, sections 1.3-1.5 describe how I have utilised the model. In section 1.3, I will very briefly show why I have adopted the specific analytic categories of individual values to analyse the development of the early periodic systems. (I reserve the detailed argument in favour of specific understanding of simplicity, completeness and carefulness to chapter 6 after the historical evidence has been introduced.) Apart from individual values, I also show how the hermeneutic model requires updating philosophical analyses of the more collective classes of values (e.g. epistemic, political, aesthetic values). After showing why even the most promising general accounts of values does not capture the

many roles of values in the development of the periodic systems (section 1.4) I will then argue in favour of the functions framework for values (section 1.5).

1.2 What Is the Hermeneutic Model and Why Do We Need It?

From the many ways of conducting iHPS investigations, Schickore argues in favour of an approach that she titles the hermeneutic model of iHPS. In this approach, the investigator would start by adopting a set of initial analytic categories for interpreting some episode of actual science (Schickore 2011, 461). Once these categories have been first applied to analyse the episode in question, the investigator gets an impression of how well the categories elucidate that particular episode of science. After gaining such an insight, the investigator would then tweak, sharpen, or replace the initial analytic categories in the process of navigating towards an illuminating analysis of the chosen episode (Schickore 2011,461). In short, Schickore's hermeneutic model consists of *interpreting* the historical episode with the help of the initial analytic categories and then *iterating* those categories so that they fare better at elucidating the episode in question.

However, the hermeneutic model for iHPS is not the only model in the market. Schickore diagnoses that the so-called confrontation model has become the most dominant way of conducting iHPS investigations. In this model, philosophy is taken as the provider of theories about science and history as the source of data to confirm or disconfirm the philosophical theories (Schickore 2011, 463). Then, the general philosophical frameworks are confronted with historical data, where data are provided mainly through case studies (Schickore 2011, 465). A seminal example of confrontation approach is the scientific change project at the Virginia Polytechnic Institute, which fashioned the practice of philosophy of science after scientific practice (Laudan et al. 1986; Donovan and Laudan 1992; Schickore 2011, 463; Scholl 2018, 3-7).

Schickore brings up several problems with the confrontation model, but for my purposes here, it is not so crucial to argue in favour of one methodological model of iHPS in general (see Scholl 2018 for a recent defence of the confrontation model). Instead, here it suffices to show that the hermeneutic model fares best at the specific task of explication. As the goal here is to explicate the role of values in science, it is important to adopt a

methodological model that helps in achieving that aim (even though the model might have problems in some other respects).

Here, I follow Rudolf Carnap in understanding explication as the process of rendering more explicit the often vague and implicit concepts used in practical settings (Carnap 1947, 7-8; Beaney 2018). I suggest that the hermeneutic model is especially suited for explication because it provides a three-part road map for the process. First, the investigator starts by (i) adopting initial categories for interpreting some episode in history of science or current scientific practice. Once these (ii) initial categories have been applied to analyse the episode, they are then (iii) changed, tweaked, or kept on the basis of how well they explicate. In principle, it ought not to matter whether the categories derive from the historical context or elsewhere; we care about how well they illuminate the episode in question. Thus, the most critical requirement is that the initial analytic categories illuminate the historical episode, where it is more likely that the more thorough definitions of the analytic categories are the end-products of the investigation, rather than their starting-point.

The usefulness of the hermeneutic model at explication becomes especially evident when we contrast it with the confrontational model. If we used the confrontation approach, the investigator would start with a hypothesis on values, and then confront the historical sources with that hypothesis. While the confrontation model seems to be better suited for testing the generality or the scope of our analytic categories, it is not clear how it could be used for rendering explicit the vague assumptions hidden in the historical episode of interest.

However, there is at least one compelling counter-argument that the defender of the confrontation model might make against the hermeneutic approach. She might argue that although their arguments may *appear* to use the confrontation approach by first introducing the philosophical thesis, and then providing a historical case, in fact, they could have applied the hermeneutic model in the background. This is because in the initial process of establishing their theses, the defenders of the confrontation model may have iterated their preliminary philosophical hypotheses on the basis of the first application to

the episode. After such initial iteration, they could conceal the changes and only show the final results: the updated philosophical thesis which is nicely supported by a case study. In short, while the presentation of their arguments might look like conforming to the confrontation approach, behind the scenes they may have been hermeneutic all along.

I am not in the position to generalise to which extent this might be the case with works that appear to endorse the confrontation approach. However, I think an important part of executing the hermeneutic approach is to *show* how the initial analytic categories have been changed or iterated. This is not just a matter of methodological sectarianism or signalling who has followed one model most thoroughly. I suggest that transparency over the processes of iterating the analytic categories allows to assess how investigators have navigated the (at times difficult) negotiations between analytic categories and the sources. In particular, transparency allows to see whether the reasons for keeping or changing the categories have been compelling or not. For that reason, the rest of this chapter is dedicated to demonstrating how the hermeneutic model has been used in the course of this thesis. More specifically, I show how the analytic categories have been changed in order to explicate better the role of values in the systematisation of the chemical elements.

1.3 Applying the Hermeneutic Model in Practice

In the previous section, I outlined that Schickore's account of the hermeneutic model gives us a three-step map for explication. However, I also suggested that it would be helpful to be more transparent about the process of applying the hermeneutic model. For this reason, I suggest the following four-fold outline of the hermeneutic approach employed in this thesis:

(i) Application of the initial analytic categories to episode of actual science
(ii) Testing how well the categories explicate
(iii) Updating the categories if they fail to explicate, and
(iv) Stating how the analytic categories have been iterated or changed in the course of the investigation

As such, this section focusses on the fourth stage of the process. In particular, I show how the initial hypothesis of this thesis and the initial analytic categories have been changed or tweaked. More specifically, I will first discuss the changes to the more general working hypothesis of this thesis, the specific categories of individual values, and the more general, collective categories for values.

Working hypothesis

In the first stages of the project, I chose the values of simplicity, accuracy, and completeness as my initial analytic categories. This was because in my first assessment of the early periodic systems, it appeared that Mendeleev emphasised completeness in the course of developing his systems, and Newlands brought up simplicity repeatedly. The choice of an initial analytic category was less straightforward with Meyer, but use of more precise atomic weights, together with his attention to the uncertainty of data, seemed to speak of his valuing of accuracy. As the values shaped a practice of systematisation of the elements that appeared "internal" to chemistry, I referred to the values as epistemic.

After a more in-depth study of the secondary and the primary source material (stage ii), it became clear that my initial hypothesis which linked one value per one chemist needed some reassessment. While I remained confident that the specific values helped to interpret and articulate the differences between the systems, it also became apparent that their role was more complicated than I had initially suspected. It was not that one chemist emphasised just one value, but that there were also other values at play. For instance, Newlands emphasised simplicity, but he also sought after a complete system. After growing awareness of such nuances, I reassessed my initial working hypothesis (stage iii). Instead of linking one value per one chemist, I made it explicit when there were several values present. However, there was evidence that some values were more dominant than others. Thus, I sought to make the complexity of values explicit in the historical chapters of this thesis, but also show how chemists emphasised some values over others.

Individual values

Apart from increasing the number of values, another major revision (stage iii) concerned individual values of simplicity, accuracy, and completeness. Full arguments in favour of the

suggested definitions of the values can be found from chapter 6, but in the interest of transparency, I offer a brief overview to the changes to the initial categories.

As noted earlier, in the first stage of the hermeneutic approach, I adopted the values of simplicity, accuracy, and completeness as my initial analytic categories. More thorough study of the primary sources revealed that especially accuracy was problematic. While accuracy (understood as multiplication of observation) was gaining foothold in the mid-nineteenth century chemistry in the German lands, and was a thus a good candidate for making sense of Meyer's distinct approach to systematisation, it did not capture all of Meyer's strategies of signalling the quality of observations. Furthermore, in the current literature on philosophy of science, accuracy is often viewed as closeness to truth (e.g. Khalifa 2018, see chapter 6 for a more in-depth discussion). Such connotations risk distorting Meyer's position of scepticism about the efficiency of theories as the vehicles for truth. As will be explained in chapters 4, 6 and 8, Meyer argued against thinking that even the best theories would be true. For the above two reasons, I changed the category of accuracy to that of carefulness.

From the three values, completeness was the least troublesome. I adopted it primarily for analysing the approach of Mendeleev, but it also came up in the publications of Newlands. Although there were some clear indicators of the greater completeness of Mendeleev's periodic systems, there were also some difficulties in articulating what completeness involved for Mendeleev. While Mendeleev argued that completeness was an important requirement for a natural classification, he also did not explicitly elaborate what constituted completeness. To flesh out what completeness amounted to, I adopted Hasok Chang's (2012a) definition of the term and tested how Mendeleev's approach could be analysed with its help. After applying Chang's definition to Mendeleev's and Newlands' publications, it became clear that there were some limitations to the initial definition. I then revised Chang's definition in order to render it suitable for accounting for the concerns of mid-nineteenth century chemistry. The updated version of the definition was then more helpful at analysing both the completeness of Mendeleev's systems and for articulating why his systems were more complete than those of Newlands and Meyer.

As for simplicity, identifying it as a suitable category was straightforward, as Newlands both discussed his system's simplicity and his systems appeared visually simpler than the others. However, elaborating simplicity turned out to be challenging, as a suitable template of simplicity could not be found in the same way as with completeness. As the philosophical accounts of simplicity did not seem to capture the simplicity of Newlands's arrangements, I focussed on drawing links between the visual features of his systems and his comments concerning how he had identified a "simple relation." While it was straightforward to articulate what visual features contributed to its comparative simplicity, it was far less straightforward to elaborate what the "simple relation" boiled down to. In the end, I opted for Adrian Currie's definition of theoretical simplicity as it allowed to articulate how the specific features of the visual systems were simple (Currie 2018).

However, I also saw it as important to highlight that Currie's theoretical simplicity did not capture one of Newlands's claims concerning simplicity. After 1866, Newlands argued that his systems identified a simple relation in the world. However, as theoretical simplicity concerns predominantly simplicity of theories, it was difficult to apply Currie's account to make sense of Newlands' claim. However, interestingly, the difficulty of applying Currie's theoretical simplicity allowed to pinpoint an incoherence within Newlands's own rhetoric concerning the simplicity of his systems. While Newlands argued that his system illustrated a simple relation in the world, one of the clearest sources of its simplicity were its design features (as will be shown in chapter 6). This incoherence in Newlands' rhetoric explains why it was challenging to identify a suitable philosophical account to analyse the simplicity of Newlands's arrangements in the first place.

General Categories of Values

Earlier, I mentioned that the suitability of my definitions for simplicity, completeness, and carefulness is best appreciated after the presentation of the historical evidence (Ch. 3-5). However, I can demonstrate how the hermeneutic approach calls for updating the more collective or general categories of values (e.g., epistemic, ethical, political, and so forth) without bringing in extensive historical detail. For this reason, the rest of this chapter concerns how to update our more general categories of values.

Offering some general categories of values alongside the specific values is helpful for two reasons. Firstly, categorising the particular values of simplicity, carefulness, and completeness allows me to relate them to the philosophical debates concerning the role of values in science. Second, the general categories help to describe what kind of roles simplicity, completeness, and carefulness had more broadly. To highlight that my discussion has direct input for debates on epistemic (rather than say, ethical) values in science, I opted for calling simplicity, completeness, and carefulness epistemic values. In other words, the terminology of epistemic values specified that in my take, the three values related to knowledge rather than the potential aesthetic, political, or moral aspects of systematising the elements.

Usually, authors perceive epistemic values as different from their moral and political siblings in virtue of their connection to truthfulness (Van Fraassen 1980, 87; McMullin 1982, Longino 1997, Nolan 2014), their importance for the attainment of truth (Steel 2010), or because they constitute the truth or knowledge itself (Longino 1990; Kincaid, Dupré, and Wylie 2007, 10). However, the second stage of the hermeneutic approach suggests that the vocabulary of epistemic values is somewhat problematic. In particular, the issue is that the above philosophical articulations of epistemic values exclude features like efficiency and convenience, where especially Nolan (2014), Van Fraassen (1980, 87) and McMullin (1982) dismissed them as less-important pragmatic considerations. However, ruling out the epistemic status of convenience and efficiency is problematic when we consider the pedagogic origins of the periodic systems. In several cases, the chemists' motivation to establish a natural system of elements stemmed from the urgency of finding a more convenient classification (more on this in chapter 2). Thus, the values that initially appear to us as epistemic can equally well be characterised as pragmatic values.

Especially the completeness of Mendeleev's systems illustrates how epistemic and pragmatic considerations could merge. As Mendeleev created the first versions of the periodic system as a solution to organising a chemistry textbook for students, it would be problematic to insist that completeness was an epistemic value in the narrow sense of truth-conduciveness. This is because it was prudent for Mendeleev to cover as many elements and their properties as possible in order to help students to learn about them

more efficiently. However, it also undeniable that completeness also had a distinctly truth-attaining character for Mendeleev. To briefly anticipate the discussion in chapter 5, Mendeleev saw completeness as a requirement for achieving a natural classification, or a classification which did not only pick one feature and classify the elements with its help, but took into account many of their chemical and physical properties. A complete natural classification of the elements was viewed as an ambitious goal for chemistry, "an epitome of the whole mass of chemical knowledge existing at the time," which suggests that completeness was an epistemic value (Foster 1863,1007).

Furthermore, completeness could be viewed as an epistemic value as it supported making predictions on the properties of unknown elements. As we will see in chapter 9 of this thesis, completeness supported making predictions. Considering that predictive accuracy is the textbook example of an epistemic value (e.g., Lipton 2004, Steel 2010; Douglas 2013), the helpfulness of completeness in making predictions seems to grant it an epistemic status, too. In short, the case of completeness suggests that the same value could be characterised both as epistemic and pragmatic.

As outlined in section 1.1 of this chapter, the third stage of the hermeneutic approach calls for updating the initial general categories of values. As the second stage of the hermeneutic approach suggests that our collective accounts of values need to make sense of how one value might be simultaneously epistemic and pragmatic, I will now proceed to see if our existing accounts of values in science can make sense of the dual citizenship of values like completeness.

1.4 Testing the Accounts of Douglas and Steel

The third stage of the hermeneutic process called for revising our initial analytic categories in case they struggled to explicate the historical episode in question. In the previous section, we saw that some individual values investigated in this thesis could be characterised both as pragmatic or epistemic. In what follows, I will test how two recent accounts of epistemic values – by Heather Douglas (2013) and Daniel Steel (2010) – fare at making sense of the dual citizenship of values. I will conclude that neither quite succeeds at accounting for complex values like completeness. However, a closer

consideration of their accounts provides us with useful information on what is required for our collective take on values, so I will discuss both in detail.

The accounts of Steel (2010) and Douglas (2013) seem especially suitable for making sense of the dual citizenship of values because both aim to accommodate for the overlap between epistemic and *nonepistemic* considerations.[2] Even though neither Douglas's nor Steel's account is specifically designed to address individual value's membership in both epistemic and pragmatic categories, pragmatic values are usually viewed as nonepistemic considerations, so the accounts of Douglas and Steel provide good candidates for a suitable collective framework on values that could be employed in this thesis.

Especially Douglas's account is promising as it places pragmatic and fruitfulness considerations under the general rubric of epistemic values.[3] Douglas starts by dividing epistemic values into four different groups, where the groups rest on two distinctions. Firstly, Douglas distinguishes between epistemic values which attach to theories per se and those that attach to theory-evidence relationships. The distinction indicates whether the value attaches to the theoretical representation of natural phenomena or its evidential support. Douglas then distinguishes between values that form the ideal desiderata and minimal criteria for science. This is because it is one thing to want "one grand, simple, unified theory of great scope that explains everything", and another thing to get one in practice (Douglas 2013, 798). With these preliminary distinctions, Douglas groups epistemic values as follows (see figure 1.1).

Through providing minimal criteria for science, groups 1 and 2 give epistemic assurance (Douglas 2013, 799). Group 4 includes many of the features that we ideally would like to

[2] Both accounts of Steel and Douglas respond to Phyllis Rooney's critique of the epistemic/nonepistemic distinction (1992). Rooney draws attention to "nonstandard" epistemic values that many philosophers tend to exclude from their lists of epistemic values. Rooney sees this as inappropriate, as many so-called non-standard epistemic values have acted as epistemic considerations in the past. Such cases problematise the task of identifying clear examples of epistemic and nonepistemic values.

[3] While in this 2013 article Douglas uses the terminology of cognitive values instead of epistemic values, more recently Douglas has stated that her article mapped the terrain of *epistemic* values (see Douglas 2016, 617-18).

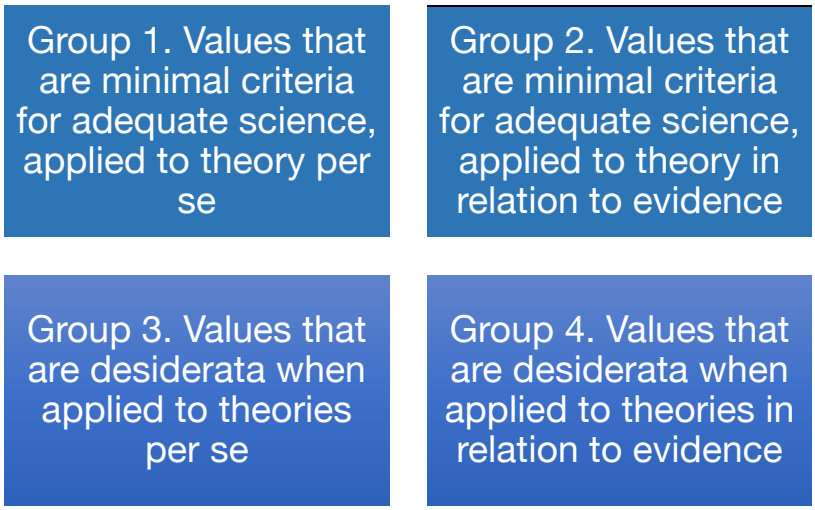

Figure 1.1 Douglas' mapping of the terrain of epistemic values.

see in a theory, so values placed there assure that "our scientific claims are more likely to be reliable, [and] that the evidence for them is in a strong form" (Douglas 2013, 800). The values of group 4 also show that the theory has genuine evidential support instead of merely reflecting our own "conceptual predispositions" (Douglas 2013, 800). In contrast to the genuine epistemic assurance afforded by group 4, group 3 includes pragmatic and fruitfulness concerns. They do not assure us of truth or empirical adequacy of the theory but make theories easier to work with.

While the inclusion of pragmatic and fruitfulness concerns under the general rubric of epistemic values (group 3) seems like a nod towards dual citizenship of values, it is still unclear whether Douglas would allow for placing values like completeness in several groups. This is because Douglas argues that the mapping is especially helpful for *separating* values into minimal epistemic criteria, pragmatic considerations, and genuine epistemic assurance (Douglas 2013, 796). For example, Douglas argues that when simplicity is instantiated by theory alone, it makes theories easier to work with, but if theory's relationship to supporting evidence instantiates simplicity, then we have a good reason think that "it is properly epistemic (i.e., indicative of truth)" (Douglas 2016, 617-618). In Douglas' words, "[w]ith this distinction in hand, one can distinguish between epistemic values (reliable guides for inference) and cognitive values (attributes of theories that make them productive or easier to use)" (Douglas 2016, 618).

This shows that Douglas's characterisation of different epistemic values does not seem to allow that a single instantiation of simplicity could be characterised both as epistemic and pragmatic. This is problematic because especially with completeness, one instantiation of the value could be characterised as having both pragmatic and epistemic import.

Apart from Douglas, Daniel Steel (2010) also offers a more nuanced take on epistemic values. Steel starts by distinguishing between intrinsic epistemic values from extrinsic ones. Epistemic value is intrinsic if "manifesting that value constitutes an attainment of or is necessary for truth" where empirical accuracy and predictive accuracy provide good examples (Steel 2010, 15). Extrinsic epistemic values promote "the attainment of truth indirectly by enhancing the efficiency of scientific enquiry" (Steel 2010, 15). With intrinsic epistemic values, Steel denies that epistemic values are *always* epistemic, because a value that appears epistemic in one context is not necessarily truth-attaining everywhere. What constitutes attainment of truth depends on "the actual state of the world, which is evidently not always and everywhere the same" (Steel 2010, 20). Furthermore, Steel argues that it would be a mistake to deny that epistemic values are social. Indeed, "[a]n epistemic value may be a social norm with a complex history, and its status as epistemic may depend on contingent facts about the world and the context of social practices within which it operates" (Steel 2010, 23). These specifications imply that Steel's account allows for overlap between epistemic, cultural and social considerations, which makes it promising for our purposes.

Despite highlighting the social nature of epistemic values, Steel's picture struggles with values being both pragmatic and epistemic. To illustrate, consider Steel's positive proposal on intrinsic and extrinsic values. Both categories hinge on truth, but Steel reassures us that he only assumes the following transparency condition: "'P' is true if and only if P" (2010, 21). While this might seem like a trivial assumption, (e.g. statement 'Ice is cold' is true if and only if ice is cold) Steel notes that this rules out the pragmatic theory of truth:

> Despite its seemingly trivial nature, the transparency condition does conflict with some theories of truth. For instance, consider a pragmatic theory of truth according

> to which whatever is useful for a person to believe is true. That this pragmatic theory of truth violates the transparency condition can be seen by examples such as the following: it is useful for Tom, who is 5 feet 10 inches, to believe that he is 6 feet tall, since this belief enhances his self-esteem. There are several theories of truth that satisfy the transparency condition, and for the present purposes there is no need to express favor for any one of them over the others (2010, 21).

Apart from the concern that Steel misrepresents the pragmatic theory of truth, I worry that ruling it out is a flaw for Steel's account more generally. In particular, assuming the transparency condition is a much more controversial matter than Steel suggests. This is because in our investigations of natural (and social) phenomena, our capabilities to satisfy the transparency condition are frequently limited. To establish that something is really the case can be a difficult, long-term goal and investigators often (but not always) have to rely on what is useful, and *assume it to be true*, in order to carry on their inquiry.[4]

For the purposes of this thesis, the most worrying issue in ruling out the pragmatic theory of truth is that doing so is bad news for values that could be characterised as having simultaneously pragmatic and epistemic import. However, the supporter of Steel's account could assure us that in the early stages of research, it is acceptable that investigators rely on pragmatic values that give no assurance of truth. Then, at the later stages, we can retrospectively confirm whether the value in question was an extrinsic or intrinsic epistemic value. Thus, we could say that at the beginning of developing the periodic system, completeness was a pragmatic concern for Mendeleev, but then turned out to be an extrinsic epistemic value.

However, this solution is not convincing for our purposes, because it requires denying the epistemic status to qualities and attributes that historical actors themselves viewed as epistemic without necessarily having the means to demonstrate that value was necessary or helpful for attaining truth. For example, in Steel's picture, Newlands' simplicity would

[4] A.S. Eddington adopted this strategy in trying to determine the internal structure of stars at a time when there was very limited empirical evidence to confirm the empirical adequacy of his hypotheses (Stanley 2007).

turn out to be a fake epistemic value, even though Newlands himself was adamant that the simplicity of his classification was a guide to its truth. Furthermore, I think it would be a serious misrepresentation of Mendeleev's own views to deny that completeness was epistemic only after finding out that predictions were highly accurate. While some might accept these kinds of concerns as the acceptable cost of endorsing Steel's account, it is not a bullet that I can bite here, as the analytic categories utilised should help to make sense of chemists' statements.

This concludes the test-trial of the accounts of Steel and Douglas. We saw that both struggle at accounting for values that are both epistemic and pragmatic. However, the reasons why the two accounts failed give us the following information: it is not enough for an account of epistemic values to allow for *movement* from one category to another, but our account needs tell how a single value may belong to several categories in a single instantiation. This is not to deny that Steel and Douglas's frameworks can be useful; for example, they may be helpful at giving us an outline on the possible roles of values in science. However, in my case, the aim is not to provide a birds-eye view of values in science but to further understanding of individual values in a specific historical context; we therefore need to find an alternative framework of epistemic and pragmatic values. This is the task for the next section.

1.5 Iterating General Categories of Values: Functions Framework of Values

The third stage of the hermeneutic process calls for revising the general categories of values so as to make sense of this dual citizenship of values. How can we make this revision? There are at least three options. Firstly, we could reject the distinction between epistemic and other kinds of values. While there are no principled articulations of this view in the literature, we can sometimes hear people reject the distinction. Usually, such rejection implies switching language of specific values ("Minimalism is an aesthetic value") to more generic language ("Minimalism is a value").

Rejecting the distinction would solve the initial issue concerning dual membership of values. However, the cost of this option is that it loses the specificity afforded by traditional categories. In this solution, we may still be able to be specific by describing that, say,

completeness was partially a pedagogic concern and also helpful for making predictions. However, I worry that giving up the vocabulary of epistemic and pragmatic categories would rule out the possibility of relating its example to existing philosophical debates concerning pragmatic or epistemic values. Indeed, if it turns out that some value may be simultaneously pragmatic and epistemic, it should be of interest for those who disagree over the primacy of epistemic over pragmatic values.

The second option for revising the general categories would be to broaden our understanding of epistemic values to include pragmatic considerations within its scope. The proponents of this solution view the dual citizenship of values like completeness as a symptom of a narrow understanding of what epistemic and pragmatic values are. However, I worry that only those who sympathise with epistemic pluralism in the first place will accept this move, and it would not convince those who endorse a narrow understanding of epistemic and nonepistemic values. For this reason, I take it that it is not enough just to make our definition of epistemic wider. A more effective strategy would be to take their narrow definitions and show that even with them, we get cases of dual membership of values. Doing so would help to show that any mutually exclusive categories fail at capturing different roles of values in historical context.

On these grounds, I think our best option for revising the general categories of values would be to keep the vocabulary of epistemic and pragmatic values, but refrain from articulating them in ways that *by default* rule out the possibility of overlap between the two (or more) categories. This route would be more desirable than the first two options, as it allows for keeping on board the helpful specificity of the vocabulary of the pragmatic and epistemic values, but also make sense of how some value can be simultaneously both.

The first step towards this solution is to give up the language of *groups* or *categories* of values. Instead of rearticulating our general categories so as they would make sense of this kind of membership, I suggest that when investigating individual values in a specific historical context, we best speak of different *functions* of values. The language of functions is well suited for describing the simultaneously epistemic and pragmatic roles of values. This is because it is easier to accept that some value has epistemic, pragmatic, aesthetic

or political functions than to accept that one value has membership in all of these categories, since more often than not they are formulated as mutually exclusive.

Let's test this solution with some examples. We saw that one of Mendeleev's motivations of making a complete system was to inject some organisation to his textbook on inorganic chemistry. This suggests the completeness of the system was a pragmatic function of aiming for convenience, but as I mentioned earlier, the system's completeness also turned out to have also an epistemic function, as completeness called for organising the elements in a way that revealed their chemical and physical similarities. If using the language of functions, there is no issue in thinking that a single value may simultaneously be pragmatic and epistemic. This thought is quite intuitive as many of our products, choices, qualities, and proposals may hide different kinds of functions. For example, my choice to attend my friend's wedding may simultaneously have the function of celebrating love, seeing old friends, or prying on the oratorical skills of her relatives. In the language of functions, there is no difficulty in the thought that a single instance of a value may simultaneously have different functions.

The language of functions helps to make sense of ambiguities that rigid categories struggle with. On these grounds, I suggest understanding epistemic and pragmatic *functions* so that they are mutually inclusive rather than exclusive. Thus, for the purposes of this thesis, I understand the epistemic function of values as follows:

> The epistemic function of values refers to those functions that historical actors see as conducive to empirical adequacy.

In this definition, I follow Bhakthavatsalam and Cartwright in understanding empirical adequacy as follows: "a theory (or model or set of scientific claims) is empirically adequate when the claims it makes about empirical phenomena – or at least the bulk of these claims, or the central ones – are correct, or approximately correct enough, where some distinction between empirical and theoretical phenomena is supposed" (2017,446). While realists might object to the vocabulary of empirical adequacy, the point here is not to argue

for the overall applicability of this definition, but to make sense of the function of values in the specific situation of systematising the elements in the 1860s.

When it comes to the pragmatic functions of completeness, carefulness, and simplicity, I define their potential pragmatic functions as follows:

> The pragmatic function of values refers to those functions that historical actors view as contributing to making the natural phenomena more understandable, intuitive, explanatory, or fruitful and convenient.

In this articulation, a pragmatic function roughly refers to the features that ease the investigator's ability to comprehend the complexity of natural phenomena and contribute to the fruitfulness of future research. It is worth noting that this definition remains silent about whether the function attaches to theories, representations, experimentation or other aspects of scientific practice and inquiry.

As advertised, the main benefit of these articulations of epistemic and pragmatic functions of values is that they allow for overlap between the two. Those functions of values that historical actors view as conducive to empirical adequacy may simultaneously also make representations of natural phenomena more understandable. Completeness may make our systems of the elements more empirically adequate, but it may also further pedagogic efficiency. Newlands saw that a simple system helped at grasping the numerical relationships between the elements and gave evidence for the existence of the relation that he had identified. Being careful at establishing a system may have the function of providing a high level of empirical adequacy, but it can also be a lesson at demonstrating the fruitfulness of error-estimation techniques for further studies on the chemical elements.

1.6 Conclusion

This thesis takes an iHPS approach to look into values in chemistry. I argued that amongst the methods of combining history and philosophy of science, the hermeneutic approach is especially useful at explicating values. Apart from arguing for the usefulness of the hermeneutic model in abstraction, I also sought to show how I have followed it in practice.

In particular, I demonstrated that the hermeneutic approach called for updating the working hypothesis of this thesis, individual categories for values, and the more general categories for values. I suggested that in the absence of suitable accounts of epistemic values, we are in the need of an alternative account, where I argued in favour of the functions framework for values.

However, this methodological chapter has only scratched the surface of the benefits of the hermeneutic approach at explicating values. Answering this question is not just the sole aim of this chapter, but the secondary aim of this whole thesis. The full benefits of the hermeneutic approach to iHPS can be appreciated better after seeing how historical research can help philosophical investigations. Demonstrating this is the task for the rest of the chapters.

Chapter 2. The Ingredients of Periodicity

2.1 Introduction

In this chapter, I provide a brief overview of the task of systematising the chemical elements in the first half of the nineteenth century. In particular, I will introduce three features that contributed to the creation of early periodic systems: atomic weights (section 2.2), the "natural groupings" of elements based on qualitative similarities (section 2.3), and the identification of regular numerical relations between atomic weights of similar elements (section 2.4). After this outline, it will become clear why the systematisations of the elements which are recognised as the precursors for the modern periodic table started rapidly emerging in different national contexts in the 1860s. Furthermore, understanding the role of atomic weights, natural groups, and the numerical relations that interested the chemists will help to illustrate how Newlands, Meyer, and Mendeleev were indeed engaging in the same task of systematising the chemical elements.

2.2. Systematisations and Atomic Weights

History of classification in chemistry is closely tied with pedagogy (Lundgren and Bensaude-Vincent 2000; Brooks 2000; Bertomeu-Sánchez, Garcia-Belmar, and Bensaude-Vincent 2002). The early periodic system of 1860s that classify the chemical elements form no exception to this trend. With respect to the chemical elements, the need for their classification did not become quite so pressing until the number of the known elements started to increase considerably.[5] In the course of the nineteenth-century chemists identified 48 new elements with the help of electrolytic techniques and spectroscopy to add to Antoine Lavoisier's list of 33 elements (Bertomeu-Sánchez, Garcia-Belmar, and Bensaude-Vincent 2002, 227; Schummer 1997).

[5] Mendeleev is known for distinguishing between elements and simple substances (Bensaude-Vincent 1986; Scerri 2005, 129; Hendry 2005; Scerri 2007, 113-115). However, the terminology of elements, simple substances, and equivalent weights was also used interchangeably. For example in Watts' Dictionary of Chemistry, the author suggested that the terms "elementary bodies, simple substances, as interchangeable to 'element', where by these terms we understand substances which cannot be, or rather which have not hitherto been, resolved into more simple forms of matter" (Anon. 1864, 482).

The newly identified elements came along with new information about their properties and a growing awareness of how the newcomers resembled previously identified chemical elements. Similarities between the elements (also known as analogies) already served as an organising principle for classifications in the late eighteenth century and they continued to do so into the nineteenth century (Bertomeu-Sánchez, Garcia-Belmar, and Bensaude-Vincent 2002, 234; Klein & Lefèvre 2007). In some systems, chemists distinguished between affinity for oxygen (Thenard 1817-1818), electric charge between the elements (e.g., Berzelius, see Bertomeu-Sánchez, Garcia-Belmar, and Bensaude-Vincent 2002, 235), metals and non-metals (e.g., Buckmaster 1867, 2-3).

Systems based on a single property of the elements were known as artificial. Such artificial systems effectively drew attention to how elements resembled each other in terms of the chosen analogy (Bertomeu-Sánchez, Garcia-Belmar, and Bensaude-Vincent 2002, 235). However, as there were many qualities that the elements shared – smell, heaviness, lightness, malleability, melting-point, acidity, basicity – such artificial classifications could also conceal other similarities between the elements. Apart from systems which focussed on one property, some chemists sought to organise the elements so that many of their analogies were expressed. Such systems were called natural classifications or systems. One example of an early debate about the helpfulness of natural systems over artificial ones comes from Louis-Jacques Thenard and Marie André Ampère. After Thenard suggested that affinity for oxygen could act as an organising principle for a systematisation, Ampère insisted that chemists should not focus on a single property but follow the lead of naturalists in aiming for a distinctly natural classification (Bertomeu-Sánchez, Garcia-Belmar, and Bensaude-Vincent 2002, 236; Ampère 1816, 296; see also Knight, 1978, 251-253). In his classification, Ampère pursued an arrangement where the properties of the elements changed gradually. This marks a similarity with Mendeleev's approach in 1869-1871, which I discuss further in chapter 5 (see Bertomeu-Sánchez, Garcia-Belmar, and Bensaude-Vincent 2002, 238-240 for the reception of Ampère's system).

As the elements had numerous properties, creating a distinctly natural classification was a demanding task. Forty years after Ampère's outline of the goals for chemical classification, the English chemist William Odling (who also engaged in the systematisation of elements) stated that aims towards natural classification required "regard, though not an equal regard, to all the properties they manifest; or in other words, we must be guided by the *totality* of their characters" (Odling 1857, 424, emphasis added). To appreciate the difficulty of this goal, consider the following statement from the chemist and physicist George Carey Foster. In his entry on classification in chemistry, Forster emphasised that

> The object of a classification of chemical substances is the arrangement of them in such a way that the position in the system of each substance may express its own chemical nature and the relation in which it stands to other substances. Hence it is easy to see that a system of classification, which should be perfect, relatively to any given stage in the development of the science, would be an epitome of the whole mass of chemical knowledge existing at the time. (Foster 1863, 1007).

In the 1860s, seven different chemists issued series of classifications that inched towards Foster's goal by bringing together quantitative and qualitative data on the chemical elements. The systems in question were issued by Béguyer de Chancourtois, Newlands, Odling, Hinrichs, Meyer, and Mendeleev (where we might also consider Heinrich Baumhauer's spiral system of 1870).[6]

Although many authors regard the systems of the aforementioned chemists as the precursors to the modern periodic table, there is an important contrast between the early systems and those employed in the twenty-first century. In particular, when we examine the modern systems, it is evident that they are organised according to atomic number rather than weight, where the number concerns the quantity of protons found in the

[6] For Chancourtois, see Scerri (2007, 68-72) and van Spronsen (1969, 97-102); for Newlands, see especially van Spronsen(1966; 1969, 102-112) and Gordin (2018); for Odling, see van Spronsen (1969, 112-116) and Scerri (2007, 82-86); for Meyer, see van Spronsen (1969, 124-132), Gordin (2012), and Boeck (2015); for Mendeleev, see especially Bensaude-Vincent (1986), Brooks (2000), Gordin (2004; 2012), and Kaji (2018); for Hinrichs, see van Spronsen (1969, 116-124); for Baumhauer, see van Spronsen (1969, 139-141).

nucleus of the atom. In contrast, the early systems were organised according to atomic weights, which can be characterised as chemically indivisible units that combine with other indivisible units of elements. Many authors on the periodic systems take the systems of the aforementioned chemists as precursors for the modern system specifically because they utilised the atomic weight data (Venable, 1896, 63; Van Spronsen 1969, 97; Scerri 2007, 66-67).

The atomic weight of each element could be only be determined in relative terms (Rocke 1984, 12; Chang 2012a).[7] To illustrate, consider the following statements from Meyer, who reminded the student of chemistry that "the weight of the atoms cannot be deduced directly from the combining proportions, and that it is only possible to decide how many times heavier or lighter one atom is than another" (Meyer 1892, 11). Meyer reassured that this did not stunt the "development of chemical theory" as knowledge of the weight of individual atoms was not required:

> The composition of any mixture of different substances can be equally well expressed in grams, ounces, or pounds, and in the same way the composition of any chemical compound can be expressed in terms of any unit of weight that may be selected. If we choose the weight of an atom of a given element as unity, we can by means of the stoichiometric values express the atomic weights of the others in terms of this standard, so that a number is obtained for each element, which shows how many times heavier it is than the unit. (Meyer 1892, 11).

Usually, the weight of a simple substance was determined in relation to hydrogen, but occasionally oxygen was used as well. The earliest atomic weight determinations were given in 1804-8 by the English Quaker John Dalton (1766-1844) who is best known for reviving the atomic hypothesis (Rocke 2005). While already in 1792-1794 Jeremias Benjamin Richter had calculated the relative weights of some compounds, it was Dalton who put forward the first weight determinations of distinctly *atomic* weights (Venable 1896,

[7] Apart from atomic weights, chemists also referred to equivalent weights. Scerri suggests that equivalent weights were regarded as a more empirical concept as they did not make explicit reference to atoms (Scerri 2007, 19). Rocke has argued that equivalent weights were as theory-laden notions as atomic weights (Rocke 1978).

14,16; on Richter, see van Spronsen 1969, 43). Unlike Richter, who studied the equivalent weights of acids and bases, Dalton investigated gases.

According to Dalton, the "greatest difficulty" arose from the fact that different gases appeared to follow "different laws" (Dalton in Venable 1896, 14). In particular, Dalton asked:

> Why does water not admit its bulk of every gas alike? This question I have duly considered, and though I am not yet able to satisfy myself completely, I am nearly persuaded that the circumstance depends upon *the weight and number of the ultimate particles of the several gases,* those whose particles are lightest and single being least absorbable, and the others more, accordingly as they increase in weight and complexity … An inquiry into the relative weights of the ultimate particles of bodies is a subject, as far as I know, entirely new (Dalton in Venable 1896, 14, emphasis added.)

Thus, from the very start, atomic weight determinations were tied to the question of why different gases have different degrees of solubility in water (Chang 2012a). According to historian Alan Rocke, Dalton's gas analysis data from 1810 suggests that water consisted of 12.5 per cent hydrogen and 87.5 per cent oxygen – exactly seven times as much oxygen as hydrogen (Rocke 2005, 126). However, experimentally identified ratios alone could not fix the weight of oxygen in relation to hydrogen. To determine the weights of oxygen and hydrogen, Dalton had to assume *how* the elements combined. Would one hydrogen atom combine with one oxygen atom, or was the formula something other than 1:1? Dalton opted for the simplest option. As Rocke explains, Dalton assumed that if

> the invisibly small water molecule consists of an atom of hydrogen united to an atom of oxygen, then every oxygen atom must weigh seven times as much as every hydrogen atom, for under these circumstances it is obvious that the weight ratio of the atoms must match the composition of the compound. (Rocke 2005, 126-127).

Assuming that the composition of water was HO, hydrogen would have the atomic weight 1, and oxygen atom 7. However, this was not the only possible solution for representing the water molecule. As Chang emphasises, this method of determining atomic weights is circular. In Chang's words:

> In the case of water, what was known from experiments was that hydrogen and oxygen always combined in 1:8 ratio by bulk weight (in approximate modern numbers). From that we can deduce that the ratio of the atomic weights is 1:16, if we know that water is H_2O, or we can deduce that the molecular formula of water is H_2O, if we know that the atomic weight ratio is 1:16. But we need to know one in order to know the other – and to begin with, we know neither. (Chang 2012a, xviii).

The example of the water molecule demonstrates how the atomic weight values required both an experiment to determine the atomic weight ratios and an interpretation of how the two elements combined. Fifty years after Dalton's first atomic weight list, these requirements had not changed. In his 1863 entry to Watts' *Dictionary of Chemistry*, Odling stated that atomic weight determination required both experimental and conceptual innovations:

> In the establishment of proportional members or atomic weights, two distinct points have to be considered, namely the exact determination of the ratios, according to which bodies combine, and the correct expression or interpretation of those ratios. The first is a question of experiment, while the second is one of judgment or inference (Odling 1863, 455).

The issue was that many interpretations were compatible with experimental results (Chang 2012a, 141).[8] The competing atomic weight systems made communication difficult, as

[8] There were other challenges facing accurate atomic weight determination. One experimental issue concerned the difficulty of determining the atomic weights of all of the elements by how they combined hydrogen, as not all elements were able to combine with hydrogen. Determining atomic weights of elements that did not react with hydrogen called for intermediate elements, which increased sources of error (Scerri 2007, 59).

"[d]ifferent chemists in different countries used different weight systems, and risked further confusion with different systems of notation" (Fisher 1982, 89). The complexity of the situation is exemplified by the fact that August Kekulé felt compelled to include 19 different formulas for acetic acid in his textbook of 1861 (Ihde 1961, 83).

Some level of consensus on atomic weights was eventually reached, however. A number of historians stress the importance of the first international conference on chemistry organised by Kekulé, Adolphe Wurtz and Carl Weltzien, which was held at Karlsruhe in 1860 (Hartley 1971, 186; Gregory 1931; DeMilt 1951, 421). In particular, such accounts tend to emphasise the importance of the Italian chemist Stanislao Cannizzaro's (1826-1910) lecture and pamphlet on standardising atomic weights, although the foundations of Cannizzaro's ideas were already laid fifty years earlier by Joseph-Louis Gay-Lussac (1778-1850) and Amadeo Avogadro (1776-1856).[9]

The Congress is often credited for resolving the issues concerning the distinction between atoms and molecules and for finding a more uniform system of atomic weights. In Meyer's later account of the value of Cannizzaro's ideas, Meyer credited Cannizzaro for being "the first to point out that an entirely false construction had been placed on the relation which exists between the density of a gas or vapour and the combining weight" even at the recognition that Avogadro had already in 1811 "given perfectly correct instructions as to the manner in which this relationship was to be employed" (Meyer 1892, 30). After growing aware of Gay Lussac's law of combining volumes, Avogadro hypothesised that "under similar conditions of temperature and pressure, equal volumes of gases contain the same number of particles, which need not of necessity be atoms. He called these particles 'molecules,' from *molecula*, a small mass (*moles*)" (Meyer 1892, 30-31, emphasis original).

Apart from Meyer, both Newlands and Mendeleev recognised the helpfulness of Cannizzaro's hypothesis and his atomic weight system. Mendeleev, who was 26-year old post-doctoral student at the time of the congress, reported its achievements as follows:

[9] For an outline of accounts stressing importance of Karlsruhe, see Chang (2012a, 142). For accounts that emphasise how the most pressing issues were mainly solved before Karlsruhe Congress see Rocke (1984, 295-96) and Chang (2012a, 146).

> The third session, September 5, was devoted to the question of atomic weights, chiefly carbon: whether to accept the new weight of 12 or remain with the former one of 6, until then used by almost everyone. After a long debate, at its last session, September 6, J. Dumas made a brilliant speech proposing to use the new atomic weight only in organic chemistry leaving the old for inorganic. Against this Cannizzaro spoke heatedly, showing that all should use the same new atomic weight. There was no vote on this question, but the great majority took the side of Cannizzaro. (Mendeleev in DeMilt 1951, 422).

Although Newlands did not use Cannizzaro's weights in his earliest arrangements, he later noted that his findings constituted an argument in favour of Cannizzaro's system of weights. In 1872, after the general acknowledgement of the discoveries of Meyer and Mendeleev, Newlands asked the editor of *Chemical News* to "vindicate his priority" in discovering the "Relations between Atomic Weights of Cannizzaro" (Newlands 1872, 252). This was because "it was only with the atomic weights of Cannizzaro that such extremely simple relationship could be observed, thereby constituting an independent argument in favour of this system of atomic weights" (Newlands 1872, 252).

The standardisation of the atomic weights, and the use of Cannizzaro's atomic weight system specifically, is often evoked as an explanation to the rapid arrival of early periodic systems of elements (Van Spronsen 1969,1; Scerri 2007, 64). However, as I noted earlier, the systems did not just make use of the quantitative data such as atomic weights, but also conveyed qualitative similarities or analogies between the elements. In the next section, I will outline how chemists identified such groups of elements that resembled one another.

2.4. Natural Groups of Elements

As we saw, classifications which divided metals and non-metals give us an example of systems that were based on qualitative properties. Apart from the division between metals and non-metals, there was also a rich tradition of identifying other groups of resembling elements, where the clusters that chemists identified were often termed "natural" groups. For chemistry teachers, identification of such groups provided one solution to the

challenge of introducing students to increasingly complex information on chemical elements and their compounds (Bertomeu-Sánchez, Garcia-Belmar, and Bensaude-Vincent 2002, 234; Bensaude-Vincent 1986, 5). Many chemists who provided periodic systems in the 1860s were interested in such similarities and provided updated groupings of their own (Odling 1857; Newlands 1863; for Mendeleev's interest in similarities, see Kaji 2018, 221).

As noted earlier, the identification of natural groupings was difficult because there were several ways in which elements could resemble each another. Although some groups of analogous elements were more widely recognised than others, the negotiations on which elements should be grouped together were still alive and well in the 1860s. To illustrate such negotiations, I will briefly introduce the natural groups that Odling put forward in 1857 and contrast them with the groupings offered by an anonymous author in Watts's *Dictionary of Chemistry* (Anon. 1864). The comparison will show how the focus on different properties led authors to suggest different natural groups.

In the introduction to his groupings, Odling noted that it was a "time-honoured doctrine in chemical science" that elements shared some properties (1857, 423). However, Odling saw that the chemists had not expressed sufficient interest in the "extent of the association, nor availed themselves of it as a means of classification" (1857, 423). Odling regarded such lack of interest as problematic:

> [A]lthough the groupings of elements are as real and certain as the natural families of plants and animals, yet we find constantly, in our systematic treatises, that bodies manifesting the strongest analogies are widely separated from one another, while bodies belonging to very different groups are conventionally associated. (1857, 424).

Before proposing his own groupings, Odling stated that the chemists classifying the elements had to be guided by the "totality" of the characters of chemical elements. One of the analogies that could guide the groupings were compounds of analogous composition. Especially if the analogous compounds had similar properties, one was justified in thinking

that the elements comprising the compounds belonged to the same natural groups (1857, 423-424). Furthermore, if one succeeded in finding “general accordance in properties” Odling advised that one had to “overlook a discrepancy in some one particular set of properties, or regard it as pertaining only to the imperfect state of our knowledge” (1857, 424). In short, some differences in the one group could be tolerated, if the elements resembled each other in properties that were deemed as of primary importance.

Odling identified four groups of elements. He included fluorine, chlorine, bromine, and iodine in the first group on the grounds that they combined with hydrogen in the proportion of “atom to atom, gaseous atomic volume to atomic volume” unlike the other elements (1857, 425). Furthermore, the elements also provided corresponding oxides and powerful acidic compounds (for further similarities, see 1857, 425-426). Odling then placed oxygen, sulphur, selenium, and tellurium in group II as the atom of each element united with two of hydrogen, and formed compounds that were neutral or “feebly acidic” (1857, 426-427, see 1857, 428 for further similarities).

Odling then grouped nitrogen, phosphorus, arsenic, antimony, and bismuth in group III, as all these elements combined with three atoms of hydrogen and chlorine. Although from the group, bismuth was not known for providing “a hydrogenized compound” (hydride) the first four elements of the group were distinctive in their capability of forming “terhydrides” (hydride containing three hydrogen atoms) that had an alkaline character instead of an acidic one (1857, 430). Odling also directed attention to the “teroxides” formed by nitrogen, phosphorus, arsenic, antimony and bismuth, and pointed that they were most “uniform” in their properties. In particular, Odling highlighted the gradual change from acidity to basicity, as the teroxides of nitrogen and phosphorus were “powerful anhydrous acids.” In contrast, the teroxide of arsenic was weaker acid. It was then followed by antimony, which was even weaker in its acidity, and bismuth, which was already basic (for further details, see 1857, 432-435). As we will see in chapter 5, this kind of information on the gradual changes in acidity was also an essential guide in Mendeleev’s organisation of his periodic system.

Finally, Odling placed boron, silicon, titanium and tin in Group IV. The resemblances were especially striking with boron and silicon, and many of their compounds were highly similar. More specifically, Odling drew attention to how both were combustible, became denser in heat, and appeared as "soft brownish powders, staining the fingers" (1857, 436). In addition, all members of the group were "remarkable for their fluorine compounds" and appeared to combine with four atoms of fluorine (see 1857, 346-7 for further details).

The above examples illustrate the kinds of similarities (and arguments) that Odling put forward in favour of his groupings. However, when we contrast Odling's groups with other proposed natural groupings, it becomes evident that not all chemists regarded the abovementioned properties as those that should be guiding natural groups. For example, the anonymous author of Watts' dictionary entry on elements disagreed with some of Odling's primary analogies (Anon. 1864, 842). Although Odling's groups I and II are represented on the classification, the anonymous author placed titanium with "silicum," tantalum, and niobium in the same group (see second group from right on figure 2.1). Furthermore, in contrast to Odling, the author saw it best to place bismuth further from nitrogen, phosphorus, arsenic, and antimony (see third group from left on figure 2.1).

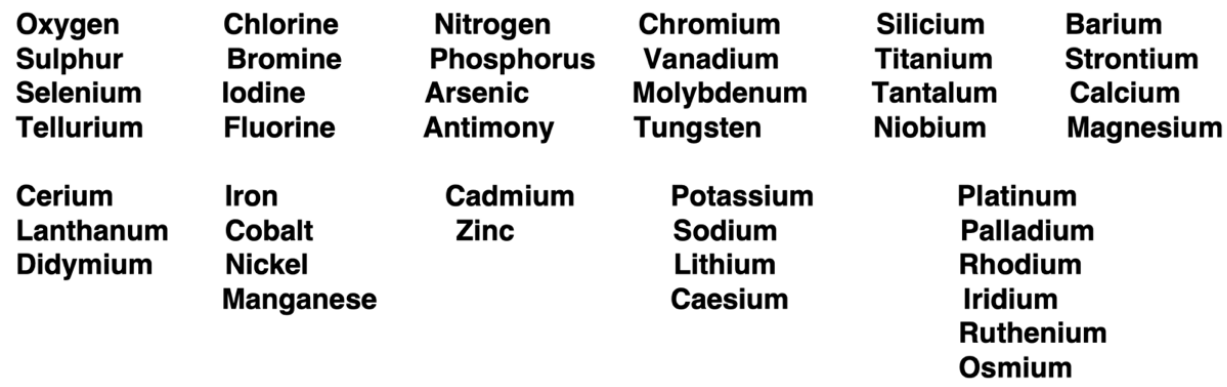

Figure 2.1 Natural groups (Anon. 1864, 842).

This brief comparison demonstrates chemists' negotiations over which properties ought to guide the identification of natural groups. As we will see in chapter 5, such disagreements were one of the reasons why Mendeleev called for a rigorous organising principle in order to systematise the elements, and was adamant that atomic weights gave such sought-after stability. In Mendeleev's explanations of the suitability of atomic weights as the organising principle, he evoked the works of chemists such as Ernst Lenssen and Peter Kremers. As we will see in the next section, Mendeleev emphasised their importance because they had identified distinct *triadic* relationships between elements in analogous groups. They also were an essential component in Newlands' systematisation process (see chapter 3).[10]

2.5 Triads

Eric Scerri's account of the history of the periodic system emphasises the importance of the identification of triadic relationships amongst the elements in the discovery of the periodic systems (2007, 57). In what follows, I will briefly show how the triadic relationships brought together qualitative and quantitative data about the chemical elements, demonstrating that that organising the elements according to atomic weight and qualitative similarities were compatible parameters for a systematisation.

Especially Newlands and Mendeleev were interested in triadic relationships formed by elements. Where Newlands sought to identify new triadic relationships, Mendeleev mostly discussed triads as a foil to his own more complete system (more on this in chapter 5). The first triadic relationships were identified by Johann Wolfgang Döbereiner, a German chemist active in Jena, who in 1817 noticed a distinct relationship in some oxides of elements (van Spronsen 1969, 63). After Döbereiner considered the oxides of barium, calcium, and strontium, it became evident that the equivalent weight of strontium was the mean of those of calcium oxide and barium oxide (van Spronsen 1969, 63; Scerri 2007, 42). In 1829, Döbereiner identified a similar triadic relationship amongst Berzelius' atomic weights, where one of the triads was formed by bromine, chlorine, and iodine. The atomic

[10] Also Odling (1857) had brought attention to some numerical relationships in his article on natural groups.

weight of bromine was the average of chlorine and iodine (see figure 2.2).

Döbereiner highlighted such numerical relations among elements that resembled one another and emphasised that triads should not just state a numerical relationship, but reveal a chemical relationship (Scerri 2007, 43). For this reason, Döbereiner primarily sought to show triadic relationships amongst elements that resembled one another. With bromine, chlorine, and iodine, the greatest similarity was that they all occurred in the sea-salts (van Spronsen 1969, 66).[11] As noted by Scerri, Döbereiner's triads can be found in the periodic system on the vertical columns (Scerri 2007, 43).

Several other chemists then adopted Döbereiner's idea of identifying triadic relationships. For example, in 1843, Leopold Gmelin constructed a whole system of triads (van Spronsen 1969, 69-72; Scerri 2007, 44-48) and by 1850, Max von Pettenkofer, who was active in the University of Munich, criticised such triadic attempts because there were triplets of similar elements that did not exhibit triadic relationships (Scerri 2007, 50). Despite his suspicions, Pettenkofer investigated the numerical differences between the elements, where he noted that the equivalent weights of lithium (Li), sodium (Na), and potassium (K) had a difference of about 16.

As we will see in chapter 4, Meyer especially highlighted such numerical differences of 16 in his first system of 1864. Furthermore, in chapter 5, I will show how Mendeleev referenced Pettenkofer as one of the chemists who influenced his own systematisation of the elements. Apart from Pettenkofer, Mendeleev also brought attention to the triadic relations of Peter Kremers, who was the first chemist to consider triadic relations among elements that appeared to have only a little in common. Scerri argues that Kremers, who referred to his findings as "conjugated triads" was one of the earliest chemists to consider "what would eventually form horizontal series of elements in the mature periodic systems of the future" (Scerri 2007, 54, 53).

[11] In modern terms, all three belong to the halogen family.

$$\mathrm{Br} = \frac{\mathrm{Cl}+\mathrm{I}}{2} = \frac{35.470+126.470}{2} = 80.470$$

Figure 2.2 From van Spronsen (1969, 66). Van Spronsen notes that there was an error in the value of 80.470 – it should read 80.970.

As noted above, the numerical differences of 16, as identified by Pettenkofer, were brought up by Mendelev and Meyer. Furthermore, I will demonstrate that the triadic relationships were especially important for Newlands (chapter 3) and Mendeleev (chapter 5). To briefly advertise, the regular numerical relationship hinted to Newlands about the existence of a "simple relation" among similar elements. In contrast to Newlands, Mendeleev used the triads of Lenssen, Pettenkofer, and Kremers as a point of contrast to his own project. While Mendeleev recognised the value of their investigations, he also viewed them as fragmented and brief. In particular, he argued that they were unsuccessful at bringing together "all of the known elements," nor did they lead to "a complete system of elements" (Mendeleev 1869b, 15, emphasis added).

2.6 Conclusion

Scerri argues that "the development of the periodic table has involved a delicate interplay between two contrasting approaches: discerning quantitative physical data, on one hand, and observing qualitative similarities among the elements as a form of natural history, on the other" (2007, 29). In this chapter, I briefly introduced the key ingredients of early periodic systems: the (quantitative) atomic weights, and the (qualitative) similarities between the elements. As we saw, triadic relations identified among chemical elements that resembled each other give us the first example of chemists bringing together the qualitative and quantitative data. In the following chapters, it will become evident that the early periodic systems brought together both qualitative and quantitative data in a more substantial manner than before (although we can regard Gmelin's triadic system as a notable exception). In particular, the creators of the systems discovered that Cannizzaro's atomic weights were compatible with expressing qualitative similarities between the elements. Their recognition of the importance of triads suggest that the triadic relationships

identified by Döbereiner, Pettenkofer, Gmelin, Lessen, and Kremers paved the way for the realisation of the compatibility of these two organising parameters. The task of the next three chapters is to show how Newlands, Meyer, and Mendeleev used the new atomic weights to express similarities between the elements and to show gradual transitioning across the systems.

Chapter 3. Newlands' Law of Octaves

3.1 Introduction

In August 1865, on the pages of *Chemical News,* the reader could find a brief article calling attention to what the author referred to as "the Law of Octaves" (Newlands 1865, 83). They would have quickly noticed that the law seemed to concern a regular numerical relationship between atomic weights of analogous elements, where the law was illustrated with a table that appeared very neat and regular. The table – and the law, for that matter – was issued by John Alexander Reina Newlands, a London-born chemist who regularly participated in the meetings of the Chemical Society (Miller 1865a, 42-43; Miller 1865b, 177; Miller 1865c, 294; Williamson 1865, 90).

Newlands' table of August 1865 was just one of the many published in 1863-1875. However, together with the slightly updated version of 1866, it represents the first more complete version of Newlands' examinations of the atomic weight relations between similar chemical elements. Although in his brief explanation of the table Newlands did not explicitly evoke simplicity, in the previous versions grasping towards the law, he emphasised having identified a "simple relation." And, as we will see, Newlands especially brought up "simple relations" both when defending his priority and when his system was accused of its overtly theoretical appearance.

Unlike with chapters 4 and 5, I will not start my account on Newlands' systems by giving a more rigorous definition of simplicity in the beginning. This is because one of the intriguing features of Newlands' approach to systematisation is that his emphasis on simplicity attaches to slightly different aspects in 1863-1864, in 1866, and in 1872-1884. It is not that the simplicity of the system would change from, say, parsimony to elegance, but that it appears to have many sides, and in different contexts, Newlands chose to focus on some over others. For this reason, the fuzziness of the term "simplicity" is going to be useful for first demonstrating how Newlands evoked simplicity in the course of developing his Law of Octaves. This will give us the grounds for then developing a more nuanced account of simplicity in chapter 6.

Apart from just evoking simplicity, I will also show how Newlands evoked *completeness* in the development of his Law of Octaves. In contrast to Mendeleev, however, completeness of Newlands' systems did not concern *both* the qualitative and quantitative properties of the elements. Rather, for Newlands, completeness mostly involved the large number of elements included in the system.[12]

The structure of this chapter is as follows. In section 3.2, I will introduce Newlands' early papers on the numerical relationships between the elements that he published between 1863-1864. After doing so, I introduce his two versions of the Law of Octaves in more detail in section 3.3. In section 3.4, I will then explain how Newlands evoked simplicity in his subsequent defences of his systematisation. In the concluding section (3.5) I will bring together the various instances of simplicity and completeness found in the course of Newlands' process of developing his system.

3.2 Relations Amongst Atomic Weights: Newlands' Systems of 1863-1864

Newlands was born in Southwark in 1837.[13] In the course of his career, Newlands worked as a teacher, analytical chemist, and as a chief chemist at a sugar refinery at Victoria Docks. He attended the Royal College of Chemistry in 1856, after which he became the assistant to the chief chemist at the Royal Agricultural Society (van Spronsen 1966, 134; W.A.T., 1898, 396). Unlike Meyer and Mendeleev, Newlands did not attend the Karlsruhe Congress. At the time of the meeting, Newlands was taking part in the fight for Italian independence (W.A.T. 1898, 396; van Spronsen 1966, 134). Upon his return, Newlands created several classifications of the chemical elements before publishing the version that became known as the Law of Octaves.

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[12] For this thesis, I have defined completeness as *" wanting to account for as many phenomena and their observed aspects as possible in a given problem area, giving special attention to phenomena that are deemed most relevant, and saying something about accommodating phenomena that are likely to enter the problem area.*" A more detailed argument in favour of this understanding is given in chapter 6.

[13] Newlands' two obituarists disagreed on his age. However, in a short notice published in *Nature,* the anonymous author states that a "search at Somerset House has revealed the existence of a birth certificate" which implied that Newlands was born in 1837 rather than 1838 (see Anon. 1962, 437).

Group 1: Metals of alkalines: – Lithium 7; sodium 23, potassium 39, rubidium 85, caesium 123; thallium 204.
Group 2: Metals of alkaline earths – Magnesium 12, calcium 20, strontium 43.8, and barium 68.5.
Group 3. Metals of earths. Beryllium 6.9, aluminium 13.7, zirconium 33.6, cerium 47, lanthanium 47, didymium 48, thorium 59.6.
Group 4. Metals whose protoxides are isomorphous with magnesia. Magnesium 12, chromium 26.7, manganese 27.6, iron 28, cobalt 29.5, nickel 29.5, copper 31.7, zinc 32.6, cadmium 56.
Group 5. Fluorine 19, chlorine 35.5, bromine 80, iodine 127.
Group 6. Oxygen 8, sulphur 16, selenium 39.5, tellurium 64.2.
Group 7. Nitrogen 14, phosphorus 31, arsenic 75, osmium 99.6, antimony 120.3, bismuth 213.
Group 8. Carbon 6, silicon 14.2, titanium 25, tin 58.
Group 9. Molybdenum 46, vanadium 68.6, tungsten 92, tantalium 184.
Group 10. Rhodium 52.2, ruthenium 52.2, palladium 53.3, platinum 98.7, iridium 99.
Group 11. Mercury 100, lead 103.7, silver 108.

Figure 3.1 Summary of Newlands' natural groups of 1863 (Newlands 1863).

In 1863, *Chemical News* published Newlands' first classification of elements, where the main aim of the article was to examine the relationship between the equivalent weights of chemical elements which belong to the same natural family (Newlands 1863, 70-72).[14] As the elements could resemble one another in different ways, Newlands specified that he prioritised chemical characteristics over physical ones. His classification consisted of eleven groups of elements (see figure 3.1).

As advertised in the title of the publication, Newlands' aim was to examine the numerical relations between analogous elements in the same group. For example, with Group 1, he identified the following relationships:

[14] Before publishing this piece, Newlands first published a system for organic compounds (Newlands 1862). In that article, Newlands issued a table demonstrating the "principal relations" between organic substances (Newlands 1862, 40). Like with the modern periodic systems, Newlands' table of organic substances revealed relations on horizontal, vertical, and diagonal lines.

1 of lithium + 1 of potassium = 2 of sodium.
1 ” + 2 ” = 1 of rubidium.
1 ” + 3 ” = 1 of caesium.
1 ” + 4 ” = 163, the equivalent of a metal not yet discovered.
1 ” + 5 ” = 1 of thallium.[15]

This table suggested that the combined equivalent weights of lithium and potassium had a regular numerical relationship with other elements of Group 1. More specifically, if we increase gradually the number of potassium from 1-5, the combined equivalent weight of lithium and potassium would appear to be the equivalent weight of other members of the analogous group (or, in the case of sodium, a multiple of equivalent weights). However, as there was no element in the group that would have the equivalent weight of 1 lithium + 4 potassium, Newlands saw it safe to assume that a metal of the equivalent weight of 163 would be discovered.

After having pointed out such relationships in other natural families of elements, Newlands summarised his findings as follows: “If we deduct the member of a group having the lowest equivalent from that immediately above it, we frequently observe that the numbers thus obtained bear a simple relation to each other” (Newlands 1863, 71). He illustrated this “simple relation” with the following table (figure 3.2).

As can be seen, the numerical difference between the element with the lowest equivalent and the one that follows it shows a difference of about 8 or 16 (with the exception of nitrogen and phosphorus on the bottom row). It was this regular difference in the

equivalents of analogous elements that was the source of the “simple relation” that Newlands brought up earlier. After having illustrated the relation with the above table, Newlands then stated how a “similar relation” was shown when “deducting the lowest member of a triad from the highest” (1863, 71). To illustrate this relation found amongst triads, Newlands introduced the following table (see figure 3.3).

[15] This comparison can be found from Newlands (1863, 71; reprinted in 1884, 2).

When he contrasted the tables, Newlands noted how the first table (figure 3.2) suggested that the difference between the element with the lowest equivalent and the next above was either 8, or 16, whereas in the triads the difference is 8x4=32 or 8x6=48 (Newlands 1863, 71).

From the point of view of values, Newlands' first examination of numerical relations between analogous elements shows his dual aims for simplicity and completeness. In the introduction to the paper, Newlands acknowledged that many of the groupings that he put forward were already known. Despite this, he saw that he had to bring such established facts together, because of "rendering it [his account] more complete" (1863, 71).

Member of group having lowest equivalent.		One immediately above the preceding.		Difference.
Magnesium	12	Calcium	20	8
Oxygen	8	Sulphur	16	8
Carbon	6	Silicon	14·2	8·2
Lithium	7	Sodium	23	16
Fluorine	19	Chlorine	35·5	16·5
Nitrogen	14	Phosphorus	31	17

Figure 3.2 Newlands' first table of 1863 (Newlands 1863, 71).

Lowest term of triad.		Highest term of triad.		Difference.
Lithium	7	Potassium	39	32
Magnesium	12	Cadmium	56	44
Molybdenum	46	Tungsten	92	46
Sulphur	16	Tellurium	64·2	48·2
Calcium	20	Barium	68·5	48·5
Phosphorus	31	Antimony	120·3	89·3
Chlorine	35·5	Iodine	127	91·5

3.3 Newlands' second table of 1863 (Newlands 1863, 71).

Furthermore, as noted earlier, Newlands referred to the numerical relationship found between elements in an analogous group as a “simple relation” (1863, 71). This shows that from the very first publication on the elements, Newlands evoked both completeness and simplicity.

The tables 3.1, 3.2, and 3.3 relied on the old equivalent numbers, but this was soon to change. In July 1864, Newlands published a second table based on Cannizzaro’s atomic weights (see figure 3.6), where he again evoked both completeness and “simple relation.” The table was presented in the course of Newlands’ response to “Studiosus,” an anonymous reader of *Chemical News* who argued that the atomic weights of elements were with few exceptions “very nearly multiples of eight” (Newlands 1864a, 59; reprinted in Newlands 1884, 5-10; see also Studiosus 1864, 11).

Newlands responded to Studiosus with a comparison between Studiosus’ observations and his own previous findings published in 1863. One can sense that Newlands was concerned about how Studiosus had not acknowledged his previous “attention to the numerical differences between the equivalents of certain allied elements, and showed that such differences were generally multiples of eight” (1864a, 59). To illustrate his own findings, Newlands put forward the following table (figure 3.4). In contrast to the previous two tables (3.2 and 3.3), it is visible that Newlands reported the numerical difference between the elements both with hydrogen and oxygen taken as the unit.

Member of a Group having Lowest Equivalent.	One immediately above the Preceding.	Difference. H=1	Difference. O=1
Magnesium 24	Calcium 40	16	1
Oxygen 16	Sulphur 32	16	1
Lithium 7	Sodium 23	16	1
Carbon 12	Silicon 28	16	1
Fluorine 19	Chlorine 35·5	16·5	1·031
Nitrogen 14	Phosphorus 31	17	1·062
Lowest Term of Triad.	**Highest term of Triad.**		
Lithium 7	Potassium 39	32	2
Magnesium 24	Cadmium 112	88	5·5
Molybdenum 96	Tungsten 184	88	5·5
Phosphorus 31	Antimony 122	91	5·687
Chlorine 35·5	Iodine 127	91·5	5·718
Potassium 39	Cæsium 133	94	5·875
Sulphur 32	Tellurium 129	97	6·062
Calcium 40	Barium 137	97	6·062

3.4 First table of 1864 (Newlands 1864a, 59).

In his assessment of Studiosus' suggestions, Newlands alluded to how Studiosus' observation on atomic weights being very nearly multiples of eight was arbitrary and depended on the chosen unit. In order to illustrate its arbitrariness, Newlands put forward a list of elements which was ordered linearly according the magnitude of atomic weights (see figure 3.5).

As can be seen, Newlands arranged the elements in a linear order according to atomic weight. Between the elements, he marked the numerical difference between the atomic weights of the first element and the subsequent one on the list. Among these differences there were no eights or multiples of eight to be found, which Newlands took as evidence against Studious' suggestion.[16]

[16] Considering that in a year's time, Newlands' Law of Octaves would bring attention to an observation that was highly similar to that of Studiosus', it becomes clear that Newlands'

H .	1		Ca .	40	1	Ce .	92	2·5	V .	137	0
Li .	7	6	Ti .	50	10	La .	92	0	Ta .	138	1
G .	9	2	Cr .	52·5	2·5	Di .	96	4	W .	184	46
B .	11	2	Mn .	55	2·5	Mo .	96	0	Nb .	195	11
C .	12	1	Fe .	56	1	Ro .	104	8	Au .	196	1
N .	14	2	Co .	58·5	2·5	Ru .	104	0	Pt .	197	1
O .	16	2	Ni .	58·5	0	Pd .	106·5	2·5	Ir .	197	0
Fl .	19	3	Cu .	63·5	5	Ag .	108	1·5	Os .	199	2
Na .	23	4	Y .	64	0·5	Cd .	112	4	Hg .	200	1
Mg .	24	1	Zn .	65	1	Sn .	118	6	Tl .	203	3
Al .	27·5	3·5	As .	75	10	U .	120	2	Pb .	207	4
Si .	28	0·5	Se .	79·5	4·5	Sb .	122	2	Bi .	210	3
P .	31	3	Br .	80	0·5	I .	127	5	Th .	238	28
S .	32	1	Rb .	85	5	Te .	129	2			
Cl .	35·5	3·5	Sr .	87·5	2·5	Cs .	133	4			
K .	39	3·5	Zr .	89·5	2	Ba .	137	4			

Figure 3.5 Second table of 1864 (Newlands 1864a, 59).

			Triad.			
			Lowest term.	Mean.	Highest term.	
I.		Li 7	+17 = Mg 24	Zn 65	Cd 112	
II.		B 11				Au 196
III.		C 12	+16 = Si 28		Sn 118	
IV.		N 14	+17 = P 31	As 75	Sb 122	+88 = Bi 210
V.		O 16	+16 = S 32	Se 79·5	To 129	+70 = Os 199
VI.		F 19	+16·5 = Cl 35·5	Br 80	I 127	
VII.	Li 7	+16 = Na 23	+16 = K 39	Rb 85	Cs 133	+70 = Tl 203
VIII.	Li 7	+17 = Mg 24	+16 = Ca 40	Sr 87·5	Ba 137	+70 = Pb 207
IX.			Mo 96	V 137	W 184	
X.			Pd 106·5		Pt 197	

Figure 3.5. The triadic system of 1864 (Newlands 1864a, 59).

engagement with the numerical relations did not proceed in a progressive, linear fashion; Newlands fluctuated between finding the numerical relations as significant and arbitrary.

After accusing Studiosus' observation of arbitrariness, Newlands abruptly switched to discussing the "most important" numerical relations between elements (1864a, 59). To illustrate these more crucial numerical relationships, he put forward a table (figure 3.5) which drew attention to triadic relationships amongst the elements.

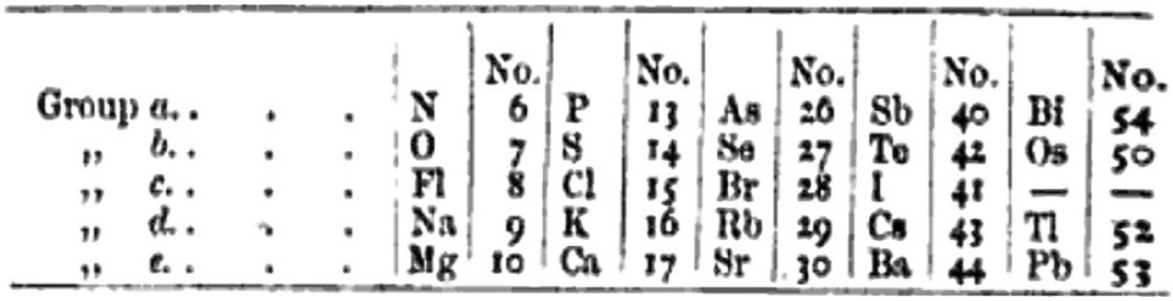
In his explanation of the table, Newlands highlighted its incompleteness as he noted it being "by no means so perfect as it might be; in fact I have some by me of a more complete character" (Newlands 1864a, 59). Newlands highlighted how the table only mapped the numerical relations between 37 elements (note that lithium features thrice and magnesium twice on the table). Reassuring the reader that he had also created a version of "a more complete character," he explained not having shared the fuller version, as "the position to be occupied by the various elements is open to considerable controversy" (1864a, 59). Instead, he published the version that contained only "little more than those elementary groups the existence of which is almost universally acknowledged" (1864a, 59). Thus, in system of 1864 Newlands had prioritised only those groups that he saw as well-established. As we will see in chapter 4, this marks a similarity with Meyer's system of 1864. In particular, limiting focus to well-known elements at the expense of completeness is one of the indicators of the valuing of carefulness.

After having explained why he had grouped the elements in groups I-X that featured on the table, the results appeared promising:

> So frequently are relations to be met with among the equivalents of allied elements, that we may almost predict that the next equivalent determined, that of indium, for instance, will be found to bear a *simple relation* to those of the group to which it will be assigned (Newlands 1864a, 60, emphasis added).

Thus, similarly to his earlier statements of 1863, Newlands' reference to "allied elements" implies that he associated the "simple relation" with the numerical relationship between analogous elements in the same group.

A month after publishing the short article debunking Studiosus' observation, Newlands put forward a new tabular arrangement of elements (1864b, 94). The new system included 24 elements and an empty space for the 25th element (figure 3.6). In the light of Newlands' previous arguments against Studiosus' suggestions, Newlands' aim for this article of August 1864 appears somewhat surprising; its intention was to illustrate how every 8th element showed similarities. However, Newlands did not use the measured atomic weights. Instead, the repetitive pattern was illustrated with *order numbers* that stood in for the atomic weights (where hydrogen was taken as the unit). Thus, for example, as nitrogen (N) is the 6th element in the list of elements, Newlands assigned it the order number 6. This observation paved the way for Newlands's famous musical analogy, which he introduced for the first time in this setting: "the eighth element starting from a given one is a kind of repetition of the first, like the eight note of an octave in music" (1864b, 94).



Group			No.		No.		No.		No.		No.
Group	*a*. . .	N	6	P	13	As	26	Sb	40	Bi	54
,,	*b*. . .	O	7	S	14	Se	27	Te	42	Os	50
,,	*c*. . .	Fl	8	Cl	15	Br	28	I	41	—	—
,,	*d*. . .	Na	9	K	16	Rb	29	Cs	43	Tl	52
,,	*e*. . .	Mg	10	Ca	17	Sr	30	Ba	44	Pb	53

Figure 3.6 (Newland 1864b, 95).

Group a…		No.		No.		No.		No.		No.
	N	6	P	13	As	26	Sb	40	Bi	54
” b …	O	7	S	14	Se	27	Te	42	Os	50
” c …	Fl	8	Cl	15	Br	28	I	41	__	__
” d …	Na	9	K	16	Rb	29	Cs	43	Tl	52
” e …	Mg	10	Ca	17	Sr	30	Ba	44	Pb	53

Figure 3.7 How Group 1, Group 2, Group 5, Group 6, Group 7 from Newlands' article of 1863 are distributed on figure 3.6. Note that in 1863, Newlands did not view lead (Pb) as an analogue of other group e members.

While for Newlands, the arrangement revealed a regular pattern in the properties of elements, unfortunately he did not elaborate what the similarities were. However, we get an idea of the similarities Newlands' alluded to if we superimpose to this system the qualitatively similar groups that Newlands identified in his earlier article of 1863. Figure 3.7 displays how the groupings of 1863 are distributed on the table.

From the eleven groups that Newlands had introduced in 1863, only the groups 1, 2, 5-7 are represented on the table. (Note that lead/Pb was not part of the original group 7, but placed to group 11, which suggests by 1864, Newlands had changed his mind about lead's analogues.) As can be seen from figure 3.7, the abovementioned groups of 1863 fit the horizontal lines of the system of 1864 quite well. The only exception is osmium (Os) which trickles from group *a* to *b*. Furthermore, Newlands reversed the position of iodine and tellurium to give precedence to chemical properties over atomic weight ordering (van Spronsen 1869, 108).

In the closing paragraph to the brief article, Newlands linked his observation of the octave to triads. Newlands suggested that if we take some elements as the centre for a triad, this allows us to identify "extremes" yet unknown (1864b, 95). This statement, together with the two empty spaces on the system, indicate that Newlands acknowledged that the table would accommodate new elements. We get a taster of using the system in this manner immediately after this article, as the editor of *Chemical News* had enclosed another short communication by Newlands. In this statement, written thirteen days after finishing the previous article, Newlands answered an inquiry concerning the atomic weight of indium. He noted that our "knowledge of the metal is too imperfect" for drawing conclusions on its equivalent (Newlands 1864c, 95). However, as the works of Henry Roscoe suggested that indium appeared similar to zinc, Newlands stated that "we should expect to find that the atomic weight of indium bears some *simple relation* to those of the zinc group, including under that term magnesium, zinc, cadmium, and, perhaps, mercury." (1864c, 95, emphasis added). Like Newlands' previous statements on the "simple relation", this, too, suggests that Newlands associated simplicity with the regular numerical relation between analogous elements. Newlands concluded the note with two predictions for the "equivalent" (i.e. the atomic weight) of indium: its weight might be very close to zinc or

cadmium, or, alternatively, if indium were taken as analogous to thallium in alkali metals, then its weight should be about 182 (1864c, 95).[17]

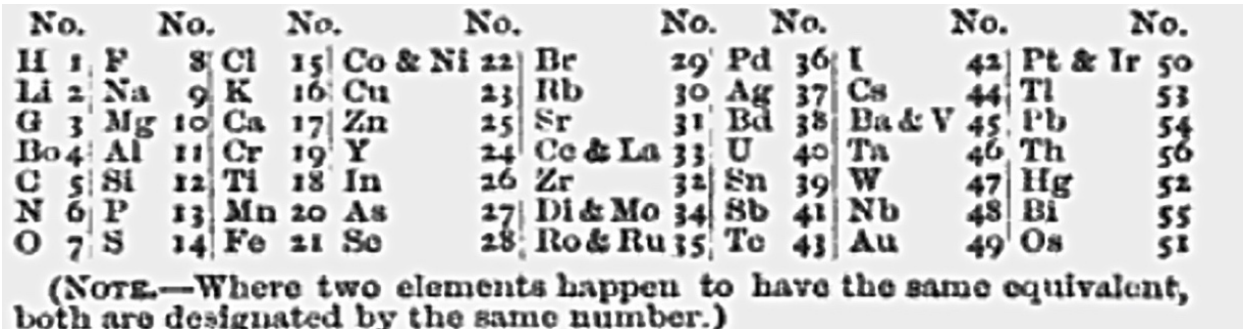
3.3 Law of Octaves: Newlands' Systems of 1865-1866

In the previous section, we saw how Newlands associated simplicity with the regular numerical relation that arose when examining the atomic weights of resembling elements. In the next year, there is a visible shift in Newlands' approach; no longer was the regular numerical relation something that arises from a group of similar elements, but it was more closely associated with the quantitative features of the elements alone.

This shift was in part a consequence of Newlands' aim towards completeness. As we saw, the system of August 1864 (figure 3.6) contained twenty-four elements. Newlands' next system (published in August 1865) expanded to include sixty-two elements (see figure 3.8). Thus, within one year, Newlands found a place for 38 additional elements, where the increase in the number of elements indicates Newlands valuing of completeness. The aim for completeness had the following consequence, however; no longer did Newlands dedicate the horizontal lines to one group only. Rather, as Carmen Giunta (1999) points out, analogical elements can be *found* from the horizontal lines. In Newlands's own words, "elements belonging to the same natural group usually appear on the same horizontal line" (1865, 83). In this way, he was able to express the numerical relationship on the horizontal lines of the system.

Like its predecessor, this system of 1865 employed order numbers that stood in for the atomic weights. Similarly to the previous versions, the new system was organised according to increasing atomic weight, albeit the monotonicity was disrupted by some inversions in all of the columns (except for the first and second ones). However, in contrast to the previous system, Newlands stated that "the numbers of analogous elements generally differ either by 7 or by some multiple of seven" where he also brought attention

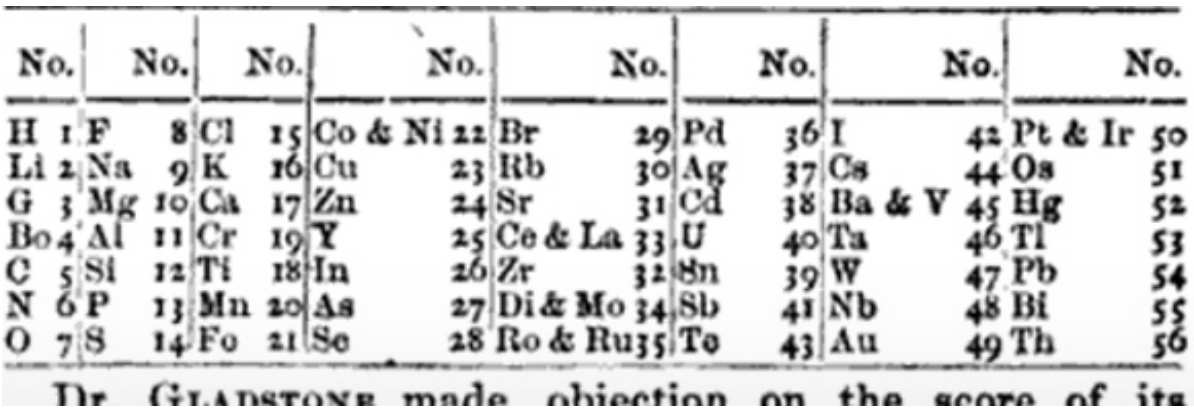
[17] In November of the same year, Newlands put forward another discussion on indium, where he suggested that it would be the mean of a triad of aluminium, indium and uranium, albeit "we must… wait for further details of the properties of this newly discovered element" (reprinted in Newlands 1884, 13).



No.		No.		No.		No.		No.		No.		No.		No.	
H	1	F	8	Cl	15	Co & Ni	22	Br	29	Pd	36	I	42	Pt & Ir	50
Li	2	Na	9	K	16	Cu	23	Rb	30	Ag	37	Cs	44	Tl	53
G	3	Mg	10	Ca	17	Zn	25	Sr	31	Bd	38	Ba & V	45	Pb	54
Bo	4	Al	11	Cr	19	Y	24	Ce & La	33	U	40	Ta	46	Th	56
C	5	Si	12	Ti	18	In	26	Zr	32	Sn	39	W	47	Hg	52
N	6	P	13	Mn	20	As	27	Di & Mo	34	Sb	41	Nb	48	Bi	55
O	7	S	14	Fe	21	Se	28	Ro & Ru	35	Te	43	Au	49	Os	51

(NOTE.—Where two elements happen to have the same equivalent, both are designated by the same number.)

Figure 3.8. The 1865 version of the Law of Octaves (Newlands 1865, 83).



No.		No.		No.		No.		No.		No.		No.		No.	
H	1	F	8	Cl	15	Co & Ni	22	Br	29	Pd	36	I	42	Pt & Ir	50
Li	2	Na	9	K	16	Cu	23	Rb	30	Ag	37	Cs	44	Os	51
G	3	Mg	10	Ca	17	Zn	24	Sr	31	Cd	38	Ba & V	45	Hg	52
Bo	4	Al	11	Cr	19	Y	25	Ce & La	33	U	40	Ta	46	Tl	53
C	5	Si	12	Ti	18	In	26	Zr	32	Sn	39	W	47	Pb	54
N	6	P	13	Mn	20	As	27	Di & Mo	34	Sb	41	Nb	48	Bi	55
O	7	S	14	Fe	21	Se	28	Ro & Ru	35	Te	43	Au	49	Th	56

Dr. GLADSTONE made objection on the score of its

Figure 3.9 The 1866 version of the Law of Octaves (Newlands 1866, 113).

to the inversions and reasserted his analogy with a musical octave (1865, 83). A year later, in 1866, Newlands presented an updated version of this system to the *Chemical Society.* The only notable difference between the version of 1865 and that of 1866 is that Newlands made the final column of the system reflect the linear atomic weight ordering (compare figures 3.8 and 3.9).

In section 3.2, I showed that Newlands's natural groupings of 1863 fitted his table of 1864 (figures 3.6 and 3.7) where I argued that Newlands associated the "simple relation" with the regular numerical relationship found amongst analogous chemical elements. However, it is crucial to note that from 1865 onwards, the "simple relation" was more closely associated with the numerical relation itself, irrespective of whether or not it arose amongst qualitatively similar elements. In order to illustrate that this is the case, let's examine how Newlands' natural families of 1863 are distributed on the systems of 1865 and 1866 (figure 3.10 and figure 3.11).

When we examine the spread of qualitatively similar groups on the two versions of the Law of Octaves, it becomes clear that Giunta's claim is correct: one horizontal line is not dedicated to one analogous group of elements, but some analogous groups can be found a horizontal line. It is noticeable how especially groups 2, 5, and 8 fit well on the horizontal lines of the system. However, superimposing the natural groups onto the system also shows that Newlands distributed some resembling elements afar from their analogues. This is especially visible with groups 4, 10, and 11. Interestingly, Newlands's choice of rearranging the final column of the system to reflect the linear ordering came with the cost that osmium (Os=51, member of group 7) was taken further away from its analogues. Furthermore, the newfound linearity forced thallium (Tl=53, member of group 1) afar from its analogues. Additionally, it is clear that the systems of 1865 and 1866 included more elements than the qualitative groups of 1863. For this reason, the reader of Newlands' article is left with the impression that for example, Bo/Be, Al, Cr, Y, Ce and La, U, Ta, and Th might be analogous with each other.

In other words, superimposing the qualitative groups of 1863 onto the systems of 1864, 1865 and 1866 shows that the snug fit between the numerical relation and the qualitative groupings in the system of 1864 is no longer present in the systems of 1865 and 1866. Instead, by 1865, the quantitative relationship became the guiding parameter in organising the systems. Another telling sign of the numerical aspects becoming the most important parameter in Newlands' systematisation is that he included double-bookings of elements. If we examine the presentation of the columns of the tables of 1865 and 1866, it is noticeable that the parts of the columns displaying the ordering of the numbers are neat, whereas the columns indicating the names of elements are filled with several elements. This shows that Newlands was adamant to fit only seven elements per column, and hold onto the neat and regular presentation of the tables even at the expense of fitting two elements with the same value to the same slot. As a consequence, the first three columns form an orderly pattern, but after that the elements are not subjected to numerical ordering; there are simply too many of them. The double-bookings allowed Newlands to retain the numerical relationship that he wanted his system to convey, and to have the analogy with the musical octave.

Figure 3.10 Distribution of the natural groups identified in the paper of 1863 in the system of 1865.

No.	No.	No.	No.	No.	No.	No.	No.
H 1	F 8	Cl 15	Co&Ni22	Br29	Pd 26	I 42	Pt&Ir50
Li 2	Na 9	K 16	Cu 23	Rb 30	Ag 27	Cs 44	Tl 53
G 3	Mg 10	Ca 17	Zn 25	Sr 31	Bd[sic.] 38	Ba&V 45	Pb 54
Bo/Be 4	Al 11	Cr 19	Y 24	Ce&La33	U 40	Ta 46	Th 56
C 5	Si 12	Ti18	In 26	Zr 32	Sn 39	W 47	Hg52
N 6	P 13	Mn 20	As 27	Di&Mo34	Sb 41	Nb 48	Bi 55
O 7	S 14	Fe 21	Se 28	Ro&Ru35	Te43	Au 49	Os 51

Group 1: “Metals of alkalines: – Lithium 7 ; Sodium 23, Potassium 39, Rubidium 85, Caesium 123; Thallium 204.” (2)

Group 2: Metals of alkaline earths – Magnesium, calcium, strontium and barium.

Group 3. Metals of earths. Beryllium, aluminium, zirconium, cerium, lanthanium, didymium.

Group 4. Metals whose protoxides are isomorphous with magnesia. Magnesium, chromium, manganese, iron, cobalt, nickel, copper, zinc, cadmium.

Group 5. Fluorine, chlorine, bromine, iodine.

Group 6. Oxygen, sulphur, selenium, tellurium.

Group 7. Nitrogen, phosphorus, arsenic, osmium, antimony, bismuth.

Group 8. Carbon, silicon, titanium, tin.

Group 9. Molybdenum, vanadium, tungsten, tantalum.

Group 10. Rhodium, ruthenium, palladium, platinum, iridium.

Group 11. Mercury, lead, silver

Figure 3.11 Distribution of the natural groups identified in the paper of 1863 in the system of 1866.

H 1	F 8	Cl 15	Co&Ni 22	Br 29	Pd 36	I 42	Pt&Ir 50
Li 2	Na 9	K 16	Cu 23	Rb 30	Ag 37	Cs 44	Os 51
G 3	Mg10	Ca 17	Zn 24	Sr 31	Cd 38	Ba&V 45	Hg 52
Bo/Be 4	Al 11	Cr 19	Y 25	Ce&La 33	U 40	Ta 46	Tl 53
C 5	Si 12	Ti 18	In 26	Zr 32	Sn 39	W 47	Pb 54
N 6	P 13	Mn 20	As 27	Di&Mo34	Sb 41	Nb 48	Bi 55
O 7	S 14	Fe 21	Se 28	Ro&Ru35	Te 43	Au 49	Th56

Group 1: “Metals of alkalines: – Lithium 7 ; Sodium 23, Potassium 39, Rubidium 85, Caesium 123; Thallium 204.” (2)

Group 2: Metals of alkaline earths – Magnesium, calcium, strontium and barium.

Group 3. Metals of earths. Beryllium, aluminium, zirconium, cerium, lanthanium, didymium.

Group 4. Metals whose protoxides are isomorphous with magnesia. Magnesium, chromium, manganese, iron, cobalt, nickel, copper, zinc, cadmium.

Group 5. Fluorine, chlorine, bromine, iodine.

Group 6. Oxygen, sulphur, selenium, tellurium.

Group 7. Nitrogen, phosphorus, arsenic, osmium, antimony, bismuth.

Group 8. Carbon, silicon, titanium, tin.

Group 9. Molybdenum, vanadium, tungsten, tantalum.

Group 10. Rhodium, ruthenium, palladium, platinum, iridium.

Group 11. Mercury, lead, silver

3.4 Defending the Law of Octaves

When Newlands presented the Law of Octaves (figure 3.9) to the Chemical Society, the audience's response was discouraging. The double-bookings of the fifth, sixth, and eighth columns appeared to be a special cause of concern. In the *Chemical News* report on the meeting, the author noted Newlands's choice of arranging "the known elements in order of succession," but also mused on his choice to place "nickel and cobalt, platinum and iridium, cerium and lanthaum, &c., in positions of absolute equality or in the same line" (Newlands 1866, 113). Gladstone, who had published classifications of his own, worried that the Law of Octaves implied that no new elements would be discovered. This is because the system did not make it evident how to accommodate new elements. The problem was especially pressing as "[t]he last few years had bought forth thallium, indium, caesium, and rubidium" where the already crowded grid implied that "the finding of one more would throw out the whole system" (Newlands 1866, 113). Gladstone's response suggests that he viewed that the system's very framework (8x7 slots) as its essential feature. As there were no vacant positions left, it was unclear how it could accommodate new elements.

Newlands' immediate response to the criticism evoked simplicity:

> I have endeavoured to describe relations actually subsisting among the atomic weights of the elements at present known, but am far from thinking that the discovery of the new elements (or the revision of the atomic weights of those already known) will upset, for any length of time, the existence of a *simple relation* among the elements … The fact that such *a simple relation* exists now, affords a strong presumptive proof that it will always continue to exist, even should hundreds of new elements be discovered. For, although the difference in the numbers of analogous elements might, in that case, be altered … the existence of a *simple relation* among the numbers of analogous elements would be none the less evident. (Newlands 1884, 19, emphasis added).

Newlands' response to the criticism implies he saw simplicity providing evidence for his successful identification of a pattern amongst resembling elements. Newlands adamantly

evoked simplicity in a similar vein in the subsequent defences of his systems. Six years after his defence of 1866, Newlands appealed to simplicity again. After the acknowledgement of the discoveries of Meyer and Mendeleev, Newlands asked the editor of the Chemical News to "vindicate his priority" in discovering the "Relations between Atomic Weights of Cannizzaro" (Newlands 1872, 252; see also Newlands 1884, 21). After all, six years earlier he had demonstrated that "it was only with the atomic weights of Cannizzaro that such *extremely simple relationship* could be observed, thereby constituting an independent argument in favour of this system of atomic weights" (Newlands 1872, 252).

An official explanation for the refusal to print the Law of Octaves was issued soon enough. A few weeks later, Chemical News published an extract from the report on the Chemical Society meeting. The extract stated that Newlands had read his paper a "Note on the Relations Among Atomic Weights" and brought attention to Meyer's publication "On the Systematisation of Organic Chemistry" where Meyer had referred to Mendeleev's work on periodicity. As Newlands's paper had been rejected from *Journal of the Chemical Society,* Newlands requested, as a "matter of justice" that his note on priority would be published. Odling, who was the president of the society at the time, explained the society "had made it a rule not to publish papers of purely theoretical nature, since it was likely to lead to correspondence of a controversial character" (Odling 1873, 318). Odling's statement, together with the immediate comments to Newlands' system of 1866 suggest that the Law of Octaves was perceived as primarily a theoretical arrangement, as its connection to empirical observations was somewhat unobvious (Gordin 2018).

Newlands continued to stress that the simplicity of the Law of Octaves gave evidence for his identification of a pattern in nature. This becomes especially clear in the preface of his 1884 book that compiled together different versions of the system. In that setting, Newlands argued that already in 1864 he had "announced the existence of *a simple relation or law* among the elements when arranged in the natural order of their atomic weights" (1884, vi, emphasis added). He evoked similar rhetoric when describing the system of 1865: "In the *Chemical News* … I published a full horizontal arrangement of the elements in order of atomic weights, and proposed to designate *the simple relation*

existing between them by the provisional term 'Law of Octaves'" (1884, vi, emphasis added). He concluded the preface by stating that "I claim to have been the first to publish a list of the elements in order of their atomic weight, and also the first to describe the periodic law, showing the existence of a simple relation between them when so arranged" (1884, vii).

3.5 "Simple relation" and completeness

In the previous section, I showed how Newlands evoked "simple relation" and completeness in the course of developing his systematisations of the chemical elements. Now that I have put forward the broader picture of Newlands's process of developing the Law of Octaves, I will briefly sum-up how his valuing of simplicity and completeness influenced the construction of the systematisation.

The first notable feature in Newlands' case is that his conception of simplicity is difficult to pin down, as he evoked slightly different aspects of simplicity from 1863 to 1883. Initially, it appears that Newlands associated "simple relation" both with the qualitative and quantitative aspects of the elements. In particular, the "simple relation" seemed to arise within the groups of analogous chemical elements, where Newlands stated that their equivalent weights "bear a simple relation to each other" (1863, 71). In a similar vein, a year later, Newlands emphasised that the numerical relation arose between the "allied" elements (1864a, 59). In his next paper, where Newlands first brought up the analogy with a musical octave, he implied that simplicity was a numerical relationship between elements resembling each other (1864b, 94).

However, by 1865, the close association of the "simple relation" with qualitative *and* quantitative aspects of the system deteriorated. Newlands' ambition of accommodating many more elements (i.e., his aim for completeness in the quantity of elements) had the consequence that the "simple relation" was no longer a numerical relationship between the "allied elements." Instead, the "simple relation" could also appear amongst elements that were not analogous. Thus, by 1865, identifying the numerical relation amongst allied elements on Newlands's table required a trained eye, as one had to distinguish between

the regular numerical relationship between the elements that resembled each other and those that did not.

To sum, whereas in 1864 the “simple relation” was more closely tied to the qualitative aspects of chemical elements, by 1865, it was more firmly a feature of its quantitative features; the musical analogy, order numbers, and double-bookings. Interestingly, a similar trend towards highlighting quantitative aspects marks also the completeness of Newlands’ systems. As we saw, in 1863 Newlands brought together different qualitative groups to render his treatment of the matter more complete. However, when we examine Newlands’ later note of the incompleteness of the system of 1864, it becomes clear that he associated the incompleteness with the low number of the elements. The fact that he sought to include more elements in the next system reinforces that completeness involved accounting for many elements. However, as we saw with the systems of 1865 and 1866, Newlands’ adherence to expressing the numerical relation had the following consequence: qualitatively similar elements were no longer on the horizontal lines. Thus, instead of aiming for completeness in accounting for the many qualities and resemblances between the elements, Newlands’ goal was to include many elements.

Following Newlands’ emphasis on simplicity and completeness gives a novel way to read his process of developing the systematisation of the elements. Although Newlands brought up the “simple relation” more frequently than completeness, it is important to recognise the interplay between the two values. Where Newlands aimed for greater quantitative completeness, consequently the “simple relation” became more strongly tied to the order numbers that stood in for the atomic weights. As we will see in the next chapter, Meyer downplayed both simplicity and quantitative completeness that Newlands had seen as important for the Law of Octaves.

Chapter 4. Carefulness and Meyer's Periodic Systems

4.1 Introduction

From the three chemists examined in this thesis, the values guiding Meyer's systematisation are the most difficult to articulate in his own vocabulary. In the course of putting forward his systematisations, Meyer did not emphasise any single term with the same frequency as Newlands evoked "simple relation" (chapter 3) or Mendeleev *polnost'* or completeness (chapter 5). However, the difficulty of pinpointing Meyer's own terms should not be taken as an implication that Meyer did not value specific attributes in the course of developing his systems. On the contrary: Meyer's approach to systematisation of the elements stands in stark contrast to those of Newlands and Mendeleev.

Unlike the other chemists examined here, Meyer consistently made explicit the quality of the observations that gave rise to his systematisations. While both Newlands and Mendeleev occasionally signalled when some observations on atomic weights were not very reliable, with Meyer, the concern over the quality of observations was a more integral part of his approach. In contrast to the other chemists, Meyer excluded elements that were not very well known, and he employed several strategies for signalling the quality of observations used in his systematisations. In short, what renders Meyer's systems distinctive in comparison with Newlands and Mendeleev, is his distinguishing between different kinds of observations.

This chapter reveals Meyer's different strategies for distinguishing between well-established and more dubious findings that then gave rise to his systematisations. As a shorthand for all these strategies, I will refer to them as the valuing of carefulness. While Meyer did not mention a single quality as frequently as Mendeleev and Newlands did, he brought up the importance of "careful" generalization and being careful (*vorsichtig gewesen*) (Meyer 1864, 9). Although a more substantial articulation of carefulness will be introduced in chapter 6, for the purposes of making the argument of this chapter easier to follow, I define the valuing of carefulness as follows:

Carefulness involves signalling differences in the quality of observations of the phenomena on the problem area, and the prioritization of good quality observations over poorer ones.

As with the other definitions of values put forward in the introductions of chapter 3 (see the first footnote of section 3.1) and chapter 5, I developed this definition only after a closer investigation of Meyer's systems. However, as with the other chemists, it was not just one value or property that guided Meyer's systematisations. As we will see, alongside valuing of carefulness he also emphasised regularity and qualitative completeness when introducing his atomic volume curve.

4.2 Königsberg Ethos

Julius Lothar Meyer (1830-1895) is most often associated with physical chemistry, but before chemistry, Meyer was more involved in medical studies. Meyer was born in Varel, Oldenburg, to a family with a long tradition in medical practice. Meyer's father was a well-known physician, and his mother a daughter of a well-known medical man, where she also assisted her husband in surgical operations (Long 1895, 664; Bedson 1896, 143). While neither Julius Lothar nor his younger brother Oskar Emil (who became later a physicist) had a career in medicine, both studied the subject (Seubert, Wislicenus, and Meyer 1911, 6). After graduating from an Oldenburg Gymnasium in 1851, Meyer started his medical studies at the University of Zürich (Seubert, Wislicenus, and Meyer 1911, 6). In 1853, he moved to Würzburg and graduated as a doctor of medicine in 1854.

In 1853, Meyer grew interested in Robert Bunsen's gas analysis work. The interest took him to Heidelberg, where Meyer spent four months working in Bunsen's laboratory (Seubert, Wislicenus, and Meyer 1911, 6). During his stay at Heidelberg, Meyer's interests broadened from medicine to physiological chemistry and physical chemistry, where the lectures of Gustav Kirchhoff introduced Meyer to "the mathematical treatment of scientific questions" (Bedson 1896, 1406). In autumn of 1856, the same year, Meyer followed his brother Oskar Emil to Königsberg (Kaliningrad) to enrol in Franz Neumann's seminar on mathematical physics, which Oskar Emil was already attending (Benfey 1981,

347; Olesko 1991, 230). During the 1.5 years spent in Königsberg, Meyer finished writing his dissertation on carbon monoxide in blood, which the faculty of philosophy in the University of Breslau (Wrocław) accepted as his doctoral dissertation (Bedson 1896, 1406). In February of 1859, Meyer was appointed as *Privatdozent* of chemistry and physics in the University of Breslau where he remained until 1866. Despite his new appointment, Meyer was listed as one of the faculty members attending Neumann's Königsberg seminar (Olesko 1980, 392).

The existing historical accounts on Meyer's periodic systems rarely elaborate on the influence of Neumann's seminar on his systems beyond the short statement that Meyer had a more mathematical training than most of his colleagues (e.g., Bedson 1896, 1423). The absence of accounts that track the influence of the Königsberg seminar on Meyer's periodic systems is surprising because Neumann sought to instil his students with a number of professional habits or sensibilities (Olesko 1991) which characterise Meyer's approach to systematisation.[18] When examining Meyer's works on the systematisation of the elements (as I do in sections 4.3-4.4), it becomes evident that the Königsberg ethos of error identification is also visible from Meyer's periodic systems. However, before I can show this, we need to take a closer look at the professional habits that Neumann sought to teach his students. My discussion in this section relies mainly on Kathryn Olesko's (1991) detailed account of the seminar.

By the time Meyer joined the *Mathematisch-Physikalisches* seminar in 1856, it had already been running for 22 years. Its organiser, Franz Ernst Neumann (1798-1895) was a German mathematician, physicist, and a mineralogist, who was mostly active in Königsberg (Chisholm 1911). According to Olesko, Neumann's aim in the seminar was to teach his students methods of original research, which included especially ways to evaluate the reliability of experimental data. Such evaluative techniques included

> quantitative techniques associated with exact experiment: the analytic determination of constant (or systematic) errors; the computation of accidental (or

[18] For sensibilities or mental habits, see Olesko (1995, 103-34), Thomas (1983; 15-16) and Chartier (1988; 24-25).

> random) errors by the method of least squares; and other methods for processing data, such as graphs … and approximation techniques. (Olesko 1991, 12).

Olesko notes that the historical significance of the seminar arises from its role in institutionalising mathematical physics. However, mathematisation of physics in German lands did not just involve the acceptance of French theories in mathematical physics or Georg Simon Ohm's theory of galvanic circuit; instead, it also involved the acquisition and dissemination of mathematical *methods* that were employed in these works (1991, 12). Part and parcel of such methods was the introduction of techniques tailored for assessing accuracy, where one of the new mathematical techniques introduced to German physical investigations in the 1830s included strategies for error determination (1991, 12).

In particular, Neumann's seminar sought to demonstrate that theories could be helpful in identifying errors. However, the first pedagogical step in demonstrating the helpfulness of theories at estimating errors in empirical results was to teach students to express physical ideas mathematically. When Meyer joined Neumann's seminar in the autumn of 1856, the new students were instructed to work with simple instruments to gain competency in addressing physical problems in mathematical terms (Benfey 1981, 347; Olesko 1991, 215). While Olesko does not detail the specific exercises for the new students in the semester of 1856-57, in 1854 the students applied various techniques including percussion pendulum, Atwood's machine, and motion in a resisting medium among others (1991, 215). In the summer semester of 1857– a year after Meyer had joined the group – the lower section of the class studied Newton's theory of resistance, the theory of collisions, and the ballistic pendulum, which were especially useful for demonstrating potential issues on the experiments (1991, 236-37).

Neumann saw that the goal of such exercises was to invite students to consider conditions in which in the "instruments are not completely fulfilled" (1991, 215). He instructed the students to assess "their influence [of the experimental conditions] on the final calculated result" which would aid them to compute constant errors of an experiment (1991, 215). Constant errors referred to regular quantitative variations in the measurements that were

caused by imperfections in experimentalists' senses or the limitations of the instruments (Olesko 1995, 108).

In the process of computing such errors, one of the most techniques was the method of least squares. Historian Stephen Stiegler (1986, 13-14) attributes its most lucid mathematical exposition to Adrien-Marie Legendre (1752-1833).[19] For a non-technical introduction, we can consider curve fitting, where the aim is to identify an optimal way to fit a curve to data points. Usually, the aim is to draw a line that minimises the distance between the values expressed by the line and the data points. The method of least squares helps to achieve this aim through minimising the quantity between the data point and the value expressed by the curve. The best way to do this is to minimise the sum of the squares formed by the data point and the value expressed by the curve. Where the sum of the squares is smallest, we also have the smallest distance between measured data and the theoretically predicted values expressed in the curve. The method allowed chemists to compute the value of accidental errors and provided an arithmetical average of the most probable result (Olesko 1995, 108). In brief, it "*approximate*[s] the desired truth" (1995, 108).

Neumann introduced the method of least squares to his seminar in the summer of 1840 when he was running the course on theoretical physics for the second time (Olesko 1991, 134). The pedagogic function of least squares was not just limited to aiding students' error analysis. Instead, closer acquaintance with the method also allowed students to examine the relationship between experimental results and theoretical estimates more closely (1991, 135). Neumann had instructed the students to calculate constant errors since 1835, but for Olesko, the introduction of the least squares was significant because it required that students computed accidental errors. This marked an epistemological turning point for the seminar, because the least squares did not just involve identifying "the material limitations of an instrument or experiment" but prepared the students "for a deeper understanding of the reliability of data, and thus of theory" (1991, 135). In short, the introduction to least squares encouraged students to "consider broad epistemological

[19] For an English translation of Legendre's passages, see Stigler (1986, 13-14).

issues rather than concentrate merely on either technical problems … or narrow conceptual issues" (1991, 136).

Thus, by the time that Meyer joined Neumann's seminar, it had already established a firm pedagogic identity in teaching students to contemplate the relationship between observation and theory, where error estimation through the method of least squares played a crucial mediatory role between the two. Apart from teaching students to express physical ideas mathematically and to contemplate the relationship between theory and data through error analysis, Neumann's seminar also prompted students to experiment with graphical methods for expressing findings. While many students used graphs to express their findings, many also found such methods difficult. According to Olesko, such difficulties arose from students' growing awareness of how "both certainty and precision were always circumscribed, and defined in terms of, the analysis of error" (1991, 255). Nevertheless, many of Meyer's peers experimented with finding visual representations for data and theory, and about ten years after his postgraduate studies so did Meyer. (I will discuss the similarities between the graphic methods used by Meyer and other Königsberg students in more detail when I introduce Meyer's atomic volume curve of 1870 in section 4.3 of this chapter).

Among the professional habits that Neumann sought to instil to his students, especially the use of the graphic methods and the deeper consideration between data and theory through error analysis characterise Meyer's approach to systematisation in his later career. After attending the Königsberg seminar, Meyer moved to Breslau, where he stayed until 1866, and then succeeded Weltzein (one of the organisers of the Karlsruhe Congress) in Karlsruhe. Meyer turned down an appointment in Königsberg, but eventually became professor in Tübingen (Bedson 1896, 1408). Soon after Meyer settled in Tübingen in 1879, Neumann sent his work on spherical harmonics to Meyer, who responded to Neumann with his recollections of

> the beautiful time twenty years ago when we listened to your lectures in auditorium… We had to exert ourselves vigorously afterwards in order to work through everything at home and derive calculations only hinted at in class. I can

> still see my brother sitting next to me in our room on Löbenicht Street as we calculated the numerical coefficients of spherical harmonics and mutually confirmed our results. Although a long time has since gone by, [this manner of teaching] appears [now] to me to be the opposite of how my students want to be taught and actually must be taught. (Meyer 1879 reprinted in Olesko 1991, 266).

Eventually, Meyer dedicated his textbook *Grundzüge der Theoretischen Chemie* (Outlines of Theoretical Chemistry) to Neumann (Meyer 1890; see also Meyer 1892).

In the next sections, I will introduce in greater detail the periodic systematisations that Meyer developed after his postgraduate days in the Königsberg seminar. In particular, I argue that Meyer's approach is characterized by his valuing of carefulness. Furthermore, I will also briefly outline how Meyer used his periodic systems to assess for errors in the empirical data (this use of the system will be discussed in greater detail chapter 8). As we will see, Meyer's use of the system to identify error echoes Neumann's ideas about the role of theories. Furthermore, I will show that Meyer's most famous representation of periodicity – the atomic volume curve – was drawn with the help of techniques employed by Meyer's fellow students at the seminar. This suggests that the Königsberg seminar also inspired Meyer to distinguish between different kinds of data in a visual format.

4.3 Meyer's Systems of 1864 and 1868

In 1860, Meyer took part in the Karlsruhe Congress. Similarly to Mendeleev, Meyer also emphasised the significance of Cannizzarro's suggestions on how to standardise the atomic weights, and how to distinguish between atoms and molecules (DeMilt 1951; Meyer 1888, 61-62). Soon after attending the Congress, Meyer started working on a textbook *Die modernen Theorien der Chemie* (1864) where he put Cannizzaro's suggestions to use. Later, in a lecture given at Plochingen in January 1885, Meyer noted that when he undertook writing the textbook in 1860, he "soon discovered that by the adoption of the new atomic weights a much greater uniformity existed in the relationships between the numbers representing the atomic weights, than had hitherto been observed" (Bedson 1896, 1414). It was in the setting of writing the textbook that Meyer introduced

two tables of chemical elements which employed Cannizzaro's standardised atomic weights.

Meyer's valuing of carefulness is already evident in the layout of these very first tabular systems and in his discussion of the tables. However, before introducing the tables in more detail, it would be appropriate to consider the broader aims of the textbook in which Meyer included the tables. Meyer's goal for the textbook was to show that theories could aid in chemical investigations. Meyer's awareness of the difficulty of pursuing more theoretical chemistry can be sensed especially in Meyer's original 1864 introduction to the textbook.[20] The running theme of Meyer's preface was Claude Louis Berthollet's attempt to introduce more theoretical principles to chemistry.[21] Although Meyer described Berthollet's contributions in a positive tone, he argued that "Berthollet believed he was nearer the stage of development which he desired to attain than was really the case" (Meyer 1864 reprinted in Meyer 1888, xxii). To Meyer, Berthollet's introduction of chemical statics "was not justified by the facts known at that time, nor indeed have later observations served to justify it" (1864 reprinted in 1888, xxii). More data needed collecting before such statements could be made:

> It was now most important for chemists to prepare, study, and classify as large a proportion as possible of those compounds the existence of which was predicted by the atomic theory. *Thus chemistry assumed more and more the form of a descriptive natural science, in which general theoretical speculations, such as those to which Berthollet had attached first importance, became now of secondary import only. This change was necessary*. As geology requires mineralogy and paleontology, as the physiology of plants and animals requires systematic botany and zoology … each speculative science requires a rich and well ordered material, if it is not to lose itself in empty and fruitless fantasies (Meyer 1864 reprinted in Meyer 1888 xxv, emphasis added).

[20] In what follows, I rely on the English language translation of the 1864 introduction, which was published in the 1888 English translation of the fifth edition of the textbook (Meyer 1888, xix-xxxii).
[21] Not to be confused with Marcellin Berthollet, who was highly suspicious of theorising in chemistry (Nye 1993).

While Meyer stressed the importance of preliminary fact-gathering before theorising, he also pointed out that even these more descriptive pursuits could not proceed without generating *some* theoretical contributions. In this context, Meyer brought up Dalton's atomic theory. After Meyer stated that no theory "has made itself so quickly and certainly felt as the atomic theory of Dalton," Meyer warned that

> With such a subject in which something new was to be expected almost day by day, every generalisation was, after a few steps, in danger of meeting some fact by which it would be either overthrown or considerably modified. *Hence the necessity for careful generalisation.* If chemistry had acknowledged and accepted every theory proposed, it would have been easily converted into a chaos from which the possibility of generalising would have entirely disappeared. (Meyer 1864 reprinted in Meyer 1888, xxvi-xxvii, emphasis added).

As we will see in this chapter and in chapter 8, the dangers of unfounded hypotheses explain why Meyer distinguished between observations of different quality and prioritised high-quality observations over poorer ones in the establishment of his periodic systems.

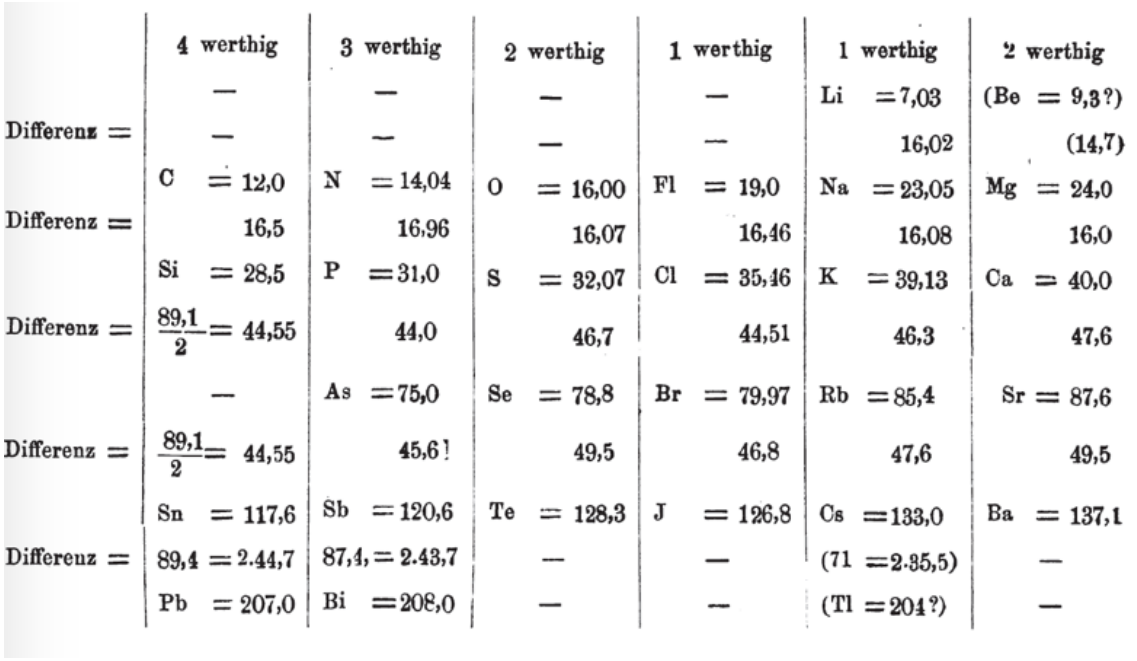
Meyer's introduction to his textbook reveals that pursuing more theoretical chemistry was not easy in the early 1860s. The difficulty of the task becomes evident when we step back from the details of the introduction and examine the general shape of his discussion. After Meyer highlighted both the value and pitfalls of Berthollet's more theoretical pursuits, he moved onto the helpfulness of Dalton's atomic theory. However, after hinting of its helpfulness, he included a warning about the risks associated with theorising. The call for caution was then accompanied by yet another hint of the helpfulness of theories:

> Having as a rule recognised this danger [the chaos caused by acceptance of wrong theories], the chemists of our century have throughout been very careful even in the formation of general theories, as also in the recognition of those theories which have been accepted at least by some chemists. *It might be more justly said that this opposition has been too strong rather than too weak*; for many of the theories now

> accepted with perfect justice had to fight in vain for years with difficulties of but little real importance. They were only accepted when this opposition had strengthened and made them more sure; others again received a tardy but un-contested recognition as the number of facts upon which they were based increased, although at first but little attention was paid to them. Some few theories only have enjoyed for a time a greater renown than they deserved or were able to support. (Meyer 1864, in 1888, xxvii, emphasis added).

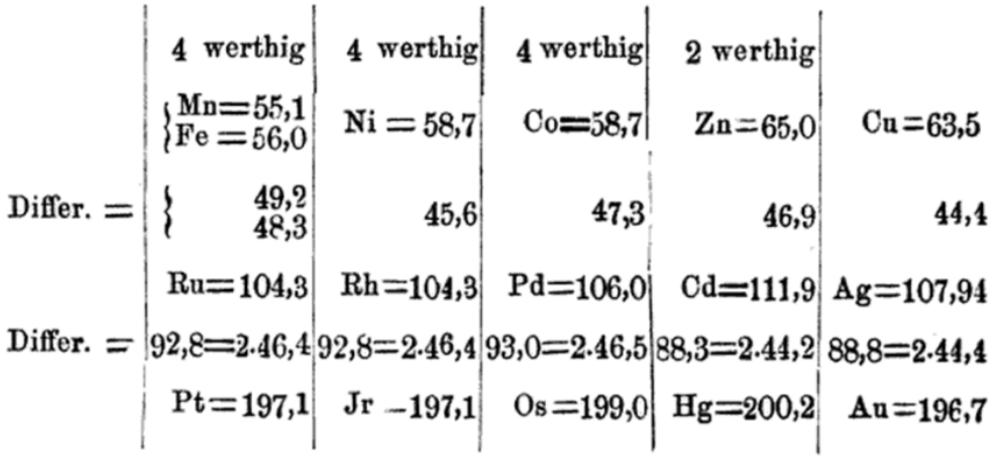
It is a testament to Meyer's skills in diplomacy in the negotiations between theory and empiricism that he thought it appropriate to push for the helpfulness of theories despite the associated risks. Meyer's balancing between stating the helpfulness of theories and warning about their dangers is also evident in the conclusion to the introduction, where Meyer stated that he did not seek to promote any one theory, but to introduce the investigators of other disciplines to the best of modern theories in chemistry – even in the light of their flaws (1888, xxxi-xxxii).

It was in this context of hinting of the helpfulness of theories that Meyer introduced his tables on the numerical relations and valency of the elements (see figures 4.1 and 4.2). Although the tables are widely regarded as the precursors to the modern periodic system, it is important to note that Meyer's goal was not to provide a system or a classification of *all* of the elements but to illustrate numerical relations between elements (Meyer 1864, 136-38). More specifically, Meyer's main aim was to use the tables to demonstrate regular numerical relations with the help of newly determined atomic weights.



	4 werthig	3 werthig	2 werthig	1 werthig	1 werthig	2 werthig
	—	—	—	—	Li = 7,03	(Be = 9,3?)
Differenz =	—	—	—	—	16,02	(14,7)
	C = 12,0	N = 14,04	O = 16,00	Fl = 19,0	Na = 23,05	Mg = 24,0
Differenz =	16,5	16,96	16,07	16,46	16,08	16,0
	Si = 28,5	P = 31,0	S = 32,07	Cl = 35,46	K = 39,13	Ca = 40,0
Differenz =	$\frac{89,1}{2}$ = 44,55	44,0	46,7	44,51	46,3	47,6
	—	As = 75,0	Se = 78,8	Br = 79,97	Rb = 85,4	Sr = 87,6
Differenz =	$\frac{89,1}{2}$ = 44,55	45,6!	49,5	46,8	47,6	49,5
	Sn = 117,6	Sb = 120,6	Te = 128,3	J = 126,8	Cs = 133,0	Ba = 137,1
Differenz =	89,4 = 2.44,7	87,4, = 2.43,7	—	—	(71 = 2.35,5)	—
	Pb = 207,0	Bi = 208,0	—	—	(Tl = 204?)	—

Figure 4.1 Meyer's first table of 1864 (Meyer 1864, 136).



	4 werthig	4 werthig	4 werthig	2 werthig	
	Mn = 55,1 Fe = 56,0	Ni = 58,7	Co = 58,7	Zn = 65,0	Cu = 63,5
Differ. =	49,2 48,3	45,6	47,3	46,9	44,4
	Ru = 104,3	Rh = 104,3	Pd = 106,0	Cd = 111,9	Ag = 107,94
Differ. =	92,8 = 2.46,4	92,8 = 2.46,4	93,0 = 2.46,5	88,3 = 2.44,2	88,8 = 2.44,4
	Pt = 197,1	Jr = 197,1	Os = 199,0	Hg = 200,2	Au = 196,7

Figure 4.2. Meyer's smaller table of 1864 (Meyer 1864, 137).

From the main table (figure 4.1), it is visible that Meyer organised the elements in ascending linear order, where the list starts from lithium (Li) and ends with thallium (Tl). Below the atomic weight values, Meyer marked the numerical difference between that element and the one situated under it. Thus, for example, the difference between lithium and sodium (Na) is 16.02. Most of the time, the numerical difference on the lighter horizontal lines is close to 16, and in the heavier lines around 44-49. In the gap below silicon (Si), we can see that Meyer interpolated the numerical difference between the

undiscovered element and its neighbour. In particular, Meyer assumed that an element with the numerical difference of 44.55 to tin (Sn) would eventually occupy the place.

Apart from the numerical relationships highlighted by the ordering, Meyer also organised the table according to valency or the combining power of the elements.[22] The valences of the elements in each column were marked on the top of the tables. For example, in the far left, we find the elements with a valency of four, which were then followed by a column of tetravalent elements. The transition of the valency from left to right appeared gradual at first, but by the columns of fluorine and lithium, the gradualness is disrupted as both columns house univalent elements. The final column on far right appeared bivalent.

Apart from the table of 28 elements, Meyer included two smaller tables. The smallest had six elements, and the larger 16 (see figure 4.2). The larger of the two included seven groups of elements that mostly had a valence of 4 (although the group on the far right was comprised of elements with different valences). It is visible that Meyer placed the heavier elements in the lower parts of the column, but the pattern was then interrupted by six inconsistencies. For example, on the left-hand side of molybdenum (Mo) 95, we find titanium (Ti) 48, but on the right – where we would expect to find an element heavier than 95 – we find manganese (Mn) 55.1 and iron (Fe) 56.0. Meyer's insertion of manganese and iron in the same vacancy marks a similarity to Newlands' system (and the very first system of Mendeleev).

While the tables aimed to highlight the regularity in the numerical relationship between the elements, the regularity is not the only distinctive feature of Meyer's representation. Both the tables and Meyer's discussion of them indicates Meyer's valuing of carefulness. Firstly, Meyer explicitly brought attention to how he only considered elements that he deemed as well-known enough (1864, 136). However, while Meyer stated that the tables considered only the well-characterised elements, it should be noted that some well-characterised elements were missing. Surprisingly, hydrogen is absent.[23] The missing

[22] According to van Spronsen, Meyer was the first person to coin the term 'valence' or *werthig* (1969, 125). For more detail on valency, see Russell (1971).
[23] I thank Hasok Chang for this observation.

hydrogen suggests that Meyer did not bring together all of the well-known elements if doing so compromised expressing the regular numerical relations between the known elements.

Apart from primarily focussing on the well-known elements, Meyer also indicated the tentativeness of some placements. Firstly, Meyer signalled the tentativeness of some placements with question marks. (Meyer was not alone in employing this technique, as Mendeleev, too used question marks.) Apart from the question marks, Meyer also brought attention to the uncertainty in the atomic weight determination of beryllium, magnesium, and thallium. In particular, "it is visible from this table that the first difference (or the first and second) on all the vertical rows amounts to almost always to 16 with the exception of beryllium and magnesium, *atomic weights of which are very doubtful*." (1864, 138, emphasis added).[24] Furthermore, on the lower horizontal lines "[b]oth of the following differences fluctuate around 46; the last difference is almost twice as high, namely 87-90, if we don't pay attention to *the not accurate enough atomic weight of thallium*, which might be slightly smaller." (1864, 138, emphasis added).[25] This shows that Meyer drew attention to the quality of data when the findings were not that well-established.

Meyer did not only signal when there were issues with the quality of observations, but also highlighted the good quality of data. In particular, Meyer made it explicit when the atomic weights of elements had been determined precisely. As can be seen on the tables, Meyer expressed some of the atomic weights in two decimal places (e.g., lithium is 7.03). The employment of such precise atomic weights was unusual. From all of the chemists who contributed to discovery of the periodic system in the 1860s, Meyer was alone in expressing atomic weights to such a high degree of precision. This suggests that Meyer wanted his table to draw attention to cases where atomic weights were determined to a

[24] "Man sieht, dass die erste (resp. die erste und zweite) Differenz in jeder Verticalreihe überall ungefähr 16 ist, ausser zwischen dem noch sehr unsicher bekannten Atomgewichte des Berylliums und dem des Magnesiums" (Meyer 1864, 138.)
[25] "Die beiden folgenden Differenzen schwanken um 46 etwa; die letzte ist annährend doppelt so gross, nämlieh 87—90, wenn wir hier wieder von dem noch nicht hinreichend sicheren Atomgewichte des Thalliums absehen" (Meyer 1864, 138).

high degree of precision, and to distinguish them from those where atomic weights were not determined quite so precisely.[26]

As to the applications of Meyer's tables, Gordin notes that it would be tempting to employ the numerical regularity to tweak the atomic weights of elements that deviated from the regularity identified by the system (Gordin 2012, 72). However, Meyer quite strongly opposed such temptations:

> It is surely not to be doubted, that a definite regularity prevails in the numerical values of atomic weights. It is rather *improbable that it is as simple as it appears,* if one leaves aside the relatively small deviations in the values of the evident differences. In part indeed these deviations can justifiably be seen as brought about through incorrectly determined values of atomic weights. But this can hardly be the case for all of them; and entirely certainly one is not justified, *as is seen all too often, to want to arbitrarily correct and change the empirically determined atomic weights due to a suspected regularity, before experiment has set a more exact determined value in its place.* (Meyer 1864 translated by Gordin 2012, 71, emphasis added)

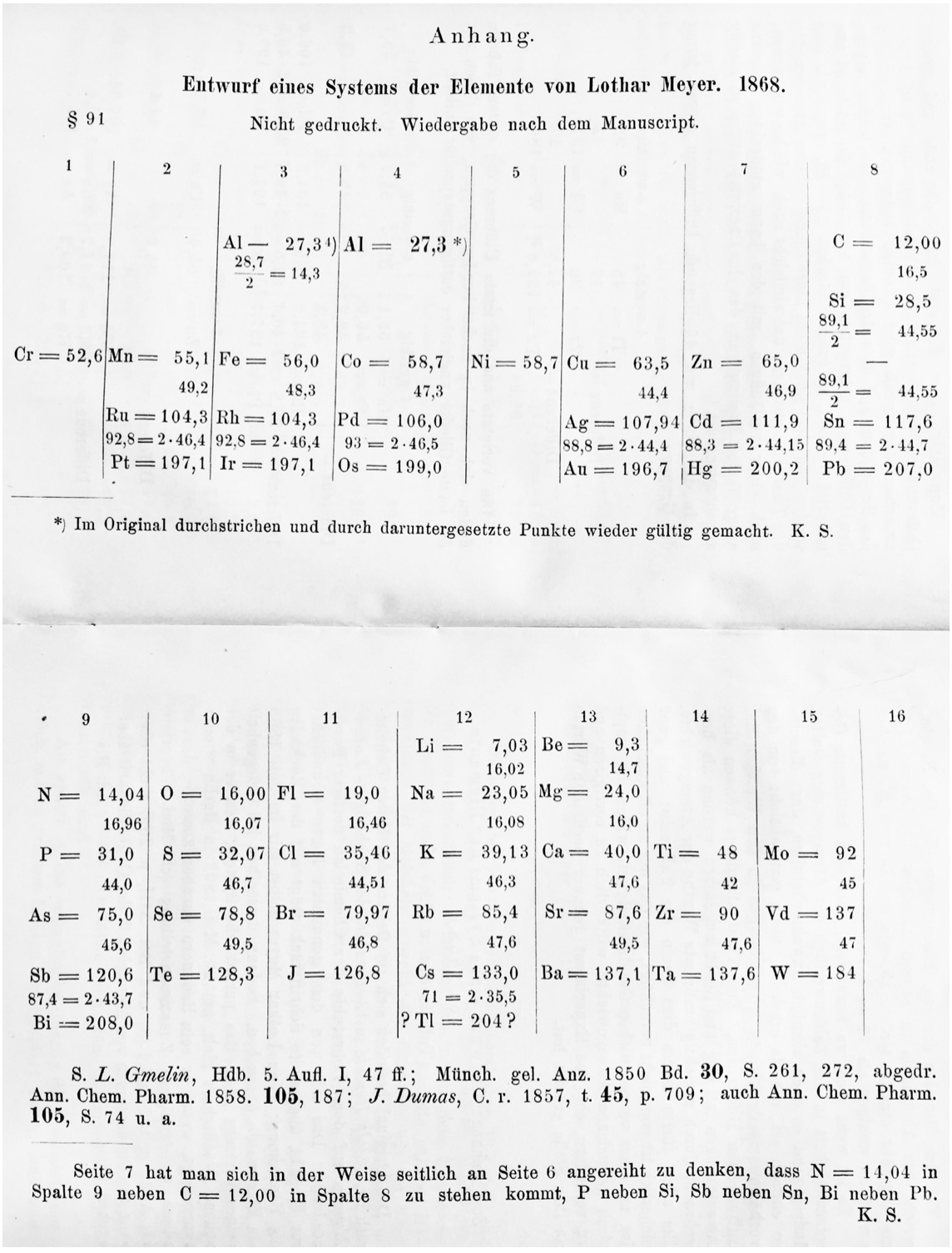
Meyer's warning against using the table to change atomic weight values suggests how the design of the tables cohered with the general aims of the textbook. In particular, more theoretical arrangements (such as regularities that emerged from individual data points) could only guide empirical investigations, not replace them. And, crucially, Meyer's prioritising of well-established observations – part and parcel of valuing of carefulness – made his system more reliable guide for future empirical inquiry.

[26] The high degree of precision in the atomic weights could be explained by Meyer's reliance on the method of least squares, as it was applied to determine atomic weights in the 1830s and 1840s (Olesko 1995, 109). However, there is no evidence for Meyer's use of the least squares. In his later life, Meyer was opposed to using the method of least squares in atomic weight determinations (Meyer 1892, 59). Neither Meyer nor his co-author relied on the method in the atomic weight determinations they issued in 1883 (Meyer and Seubert 1883).

Four years after issuing the tables of 1864, Meyer made another system that went missing for years (figure 4.3). The system was rediscovered in 1893, when Adolf Remelé, who succeeded Meyer as professor of chemistry Eberswalde, showed the table to Meyer when he was preparing for a lecture for the Chemical Society of Berlin (Bedson 1896, 1416). After Meyer's death in 1895, his collaborator Karl Seubert (1851-1942) published the missing table in a collection that included both Mendeleev's and Meyer's contributions to classifying chemical elements (Seubert 1895). The missing table was also included in Meyer's memorial lecture that was delivered by P. Phillips Bedson for the Chemical Society (Bedson 1896).

It is highly likely that this system of 1868 was intended for the second edition of Meyer's textbook. As Bedson points out, the sign "§91" indicates that Meyer sought to include it in the very section of the textbook that housed the two tables of 1864 (Bedson 1896, 1416). In contrast to the previous tables, the system of 1868 included chromium (Cr), vanadium (Vd, modern V), tantalum (Ta), and two placements for aluminium (Al).

As can be seen, on this table, Meyer's increase of the number of elements has been moderate. Instead of fifty elements, we can find 53 elements slotted to 15 columns. (Meyer left the 16th empty. Unfortunately, we do not get many clues as to why Meyer left this column vacant.) If we compare the system with the modern periodic system, it becomes evident that the table included all the elements in main and sub-groups that are present in the periodic table of today (van Spronsen 1969, 127). The only departure in the ordering according to atomic weight is Meyer's choice to place (Mo), (Vd) and (W) in the 15th column. Also, like many other chemists, Meyer was unsure about the placing of aluminium and situated it in two different locations (van Spronsen 1969, 127).



Anhang.

Entwurf eines Systems der Elemente von Lothar Meyer. 1868.

§ 91 Nicht gedruckt. Wiedergabe nach dem Manuscript.

1	2	3	4	5	6	7	8
		Al — 27,3 [4]	Al = 27,3 *)				C = 12,00
		$\frac{28,7}{2}$ = 14,3					16,5
							Si = 28,5
							$\frac{89,1}{2}$ = 44,55
Cr = 52,6	Mn = 55,1	Fe = 56,0	Co = 58,7	Ni = 58,7	Cu = 63,5	Zn = 65,0	—
	49,2	48,3	47,3		44,4	46,9	$\frac{89,1}{2}$ = 44,55
	Ru = 104,3	Rh = 104,3	Pd = 106,0		Ag = 107,94	Cd = 111,9	Sn = 117,6
	92,8 = 2·46,4	92,8 = 2·46,4	93 = 2·46,5		88,8 = 2·44,4	88,3 = 2·44,15	89,4 = 2·44,7
	Pt = 197,1	Ir = 197,1	Os = 199,0		Au = 196,7	Hg = 200,2	Pb = 207,0

*) Im Original durchstrichen und durch daruntergesetzte Punkte wieder gültig gemacht. K. S.

9	10	11	12	13	14	15	16
			Li = 7,03	Be = 9,3			
			16,02	14,7			
N = 14,04	O = 16,00	Fl = 19,0	Na = 23,05	Mg = 24,0			
16,96	16,07	16,46	16,08	16,0			
P = 31,0	S = 32,07	Cl = 35,46	K = 39,13	Ca = 40,0	Ti = 48	Mo = 92	
44,0	46,7	44,51	46,3	47,6	42	45	
As = 75,0	Se = 78,8	Br = 79,97	Rb = 85,4	Sr = 87,6	Zr = 90	Vd = 137	
45,6	49,5	46,8	47,6	49,5	47,6	47	
Sb = 120,6	Te = 128,3	J = 126,8	Cs = 133,0	Ba = 137,1	Ta = 137,6	W = 184	
87,4 = 2·43,7			71 = 2·35,5				
Bi = 208,0			? Tl = 204 ?				

S. *L. Gmelin*, Hdb. 5. Aufl. I, 47 ff.; Münch. gel. Anz. 1850 Bd. **30**, S. 261, 272, abgedr. Ann. Chem. Pharm. 1858. **105**, 187; *J. Dumas*, C. r. 1857, t. **45**, p. 709; auch Ann. Chem. Pharm. **105**, S. 74 u. a.

Seite 7 hat man sich in der Weise seitlich an Seite 6 angereiht zu denken, dass N = 14,04 in Spalte 9 neben C = 12,00 in Spalte 8 zu stehen kommt, P neben Si, Sb neben Sn, Bi neben Pb. K. S.

Figure 4.3 Meyer's system of 1868 (Seubert 1895,7).

4.4 Getting Graphic: Meyer's Systems of 1870

Two years after the missing system of 1868, Meyer published an influential article in Liebig's *Annalen der Chemie und Pharmacie* (Meyer 1870). In this article, written in December 1869, Meyer provided two new systems: one tabular and one graphic. Apart from the graphic representation of the periodic system, this article is also remembered for providing the first German-language introduction of the word 'periodicity' (*periodische Functionen)* to describe the relationship between atomic weights and properties of the elements (Meyer 1870, 358).[27]

Similarly to the introduction of the textbook of 1864, Meyer started his article with a reflection on atomic theory. For Meyer, it appeared that the regular relations between atomic weights raised questions about the internal structure of the elements. Although some authors regarded atoms as complex like molecules, others assumed they were simple. Meyer himself saw it highly likely that atoms would not be simple, but "unions of atoms of higher order, that is, groups of atoms or molecules" (1870, 354), where this idea can be seen to anticipate later developments in the atomic substructure. While Meyer regarded it likely that atoms were complex, he also stated that one *should* assume them to be made of the same primary matter. This was because

> Assuming that the atoms are aggregates of one and the same matter, *and different only by their different masses*, we may regard the properties of the elements in their dependence on the magnitude of their atomic weight, and represent them as functions of atomic weight. (Meyer 1870, 358, emphasis added).

For Meyer the assumption on the uniformity of matter was needed, because if one was to *solely* investigate the relationship between atomic weight and other chemical properties, assuming the total individuality of elements would have introduced new variables that would have rendered tracking the relationship between weight and other properties difficult. In other words, uniformity allowed one to take the atomic weights and chemical properties as the only relevant variables. The assumption of uniformity of matter then allowed Meyer

[27] Prior to Meyer's coinage, Mendeleev had used the term (*periodicheskaya*) in a Russian language publication. For further details, see Gordin (2015, Ch.2).

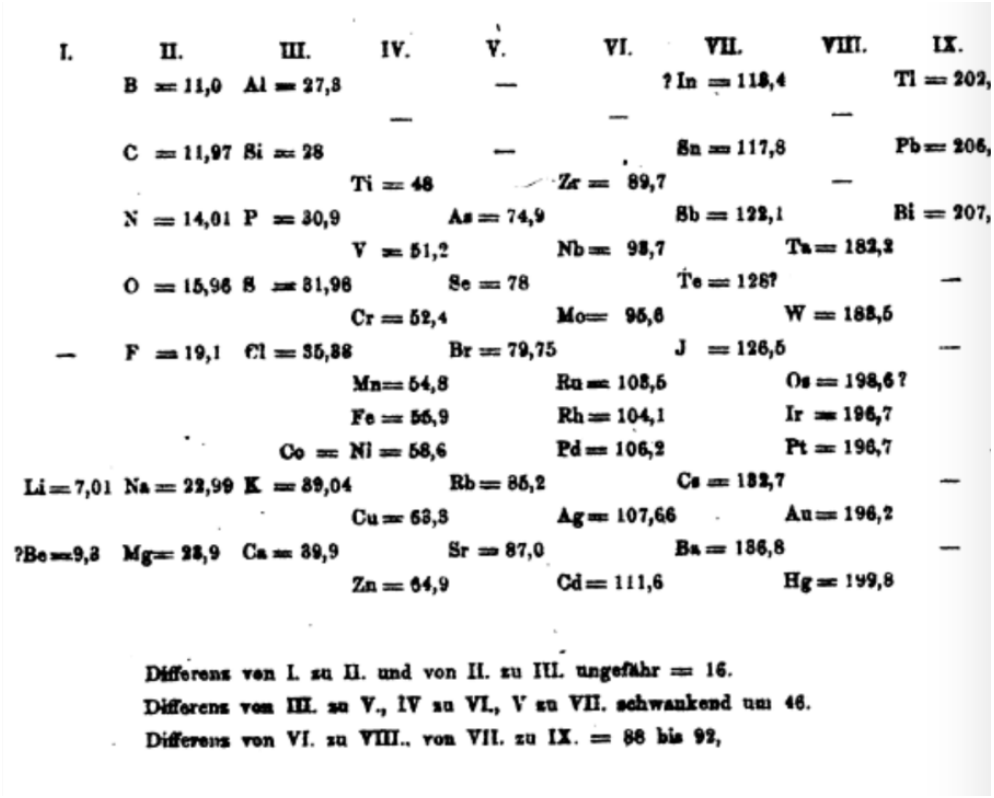
to report the most important finding of the article; that the relationship between weights and chemical properties was *periodic*.

In this context, it is crucial to note that Meyer's suggestions about theoretical assumptions are in direct contrast with Mendeleev's caution in making such statements (see Gordin 2012, 75-76). Although I have argued that carefulness reveals effectively the distinctness of Meyer's approach to systematisation, I highlight that it only attaches to the quality of observations or data. In particular, the choice of analytic category ought not to be taken as a statement about Meyer's caution concerning the role of theories. As Gordin argues, analysing the difference between Meyer's and Mendeleev's approaches in terms of timidity of the German chemist and the boldness of the Russian does not give a satisfying answer to their differing views on theories in chemistry, because Meyer was more willing to endorse theories than Mendeleev (Gordin 2012; analyses in terms of boldness or timidity can be found from Freund 1904,474; Brush 1996, 618; Boeck 2015, 48).

After such theoretical preliminaries on the uniformity of matter, Meyer briefly discussed his previous work on the relationship between atomic weight and properties of the elements. Since the past years had introduced more accurately determined atomic weights, it was possible to bring together a larger number of elements to a single classification (1870, 355). On these grounds, Meyer put forward a slightly fuller table of 55 elements (see figure 4.4), where he noted that table was similar to Mendeleev's system published as an abstract in *Journal für Praktische Chemie* in 1869.

In this system, Meyer ordered the elements according to increasing atomic weights. He focussed especially on the elements that had their atomic weights determined through investigations to their heat capacity or the gas density of their compounds (1870, 357). Apart from hydrogen, Meyer's tables excluded yttrium (Y), erbium (Eb), terbium (which Meyer marked "Tb?"), cerium (Ce), lanthanum (La), didymium (Di), thorium (Th), uranium (U), and jargonium (Jg) as neither their atomic weights nor their equivalent weights were known. The exclusion of the above elements gives us an important indicator of Meyer's prioritization of well-founded data (1870, 357). Together with the undiscovered elements, Meyer expected that such little-known elements would fill the gaps of the systems later.

In this tabular system, the columns included groups of elements that were regarded as natural families. Contrary to his earlier warning against tinkering with atomic weights on the basis of suspected regularity, Meyer changed some of the atomic weights of the elements with very close weights in order to achieve groupings according to natural families. Thus, for example, Meyer placed iodine and osmium before iridium and platinum, where all of the four stood before gold. Although in 1864, Meyer had warned against correcting atomic weights in this manner, Meyer thought such changes acceptable, because later investigations would show whether they were justified. Meyer concluded the discussion on the table by noting that "the properties of the elements are, for the most part, *periodic* functions of the atomic weight" (1870, 358, emphasis original).[28]



I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.
	B = 11,0	Al = 27,3		—		? In = 113,4		Tl = 202,
			—		—		—	
	C = 11,97	Si = 28		—		Sn = 117,8		Pb = 206,
			Ti = 48		Zr = 89,7		—	
	N = 14,01	P = 30,9		As = 74,9		Sb = 122,1		Bi = 207,
			V = 51,2		Nb = 93,7		Ta = 182,2	
	O = 15,96	S = 31,98		Se = 78		Te = 128?		—
			Cr = 52,4		Mo = 95,6		W = 183,5	
—	F = 19,1	Cl = 35,38		Br = 79,75		J = 126,5		—
			Mn = 54,8		Ru = 103,5		Os = 198,6?	
			Fe = 56,9		Rh = 104,1		Ir = 196,7	
		Co = Ni = 58,6			Pd = 106,2		Pt = 196,7	
Li = 7,01	Na = 22,99	K = 39,04		Rb = 85,2		Cs = 132,7		—
			Cu = 63,3		Ag = 107,66		Au = 196,2	
?Be = 9,3	Mg = 23,9	Ca = 39,9		Sr = 87,0		Ba = 136,8		—
			Zn = 64,9		Cd = 111,6		Hg = 199,8	

Differens von I. zu II. und von II. zu III. ungefähr = 16.
Differens von III. zu V., IV zu VI., V zu VII. schwankend um 46.
Differens von VI. zu VIII., von VII. zu IX. = 88 bis 92.

Figure 4.4 Meyer's tabular system of 1870 (Meyer 1870, 356).

[28] "Für diese Auffassung entnehmen wir aus der Tafel, das die Eigenschaften der Elemente grofsentheils periodische Funktionen das Atomgewichtes sind" (Meyer 1870, 358).

To demonstrate the periodic dependency between properties and weight more clearly, Meyer focussed on one particular quality – atomic volumes – to demonstrate its relationship with atomic weights. To illustrate their relationship, Meyer supplemented his article with a graph that tracked the relationship between atomic volume and weight (see figure 4.5).

In this graph, the x-axis displays the increasing atomic weights, whereas the y-axis maps volumes. The curve is divided into six different sections marked with Roman numerals. Furthermore, Meyer drew attention to the distribution of some qualitative properties of the elements by writing on the descending and ascending parts of the curve. This shows that Meyer was not seeking *just* to represent the periodic relationship through the atomic volume curve but used it to explore how the other properties – especially volatility, electrochemical behaviour, and ductility – were spread on different areas of the curve. By doing so, I suggest Meyer also valued completeness in accounting for qualitative properties of the elements.

Apart from exploring the spread of other properties, Meyer also used the curve for identifying errors in individual empirical results. This application will be discussed in greater detail in chapter 8, but here, it suffices to say that the divergence of some atomic volume from the regular course of the curve gave reasons to reassess the experimental results. However, such deviations did not justify changing atomic weights. This was because "[i]t would be premature, on such uncertain grounds, to make a change in previously adopted atomic weights" (1870, 364).[29] In any case, Meyer stressed that the results achieved through the use of the curve could not be expected to give as certain outcomes as experiments (1870, 364). At most, the curve could direct "our attention to dubious and uncertain assumptions and urge to re-examine them" (1870, 364).[30]

[29] "Es würde voreilig sein, auf so unsichere Anhaltspunkte hin eine Aenderung der bischer angenommen Atomgewichte vorzunehmen" (Meyer 1870, 364).
[30] "Aber sie dürfen schon jetzt unsere Aufmerksamkeit auf zweifelhafte und unsichere Annahmen lenken und zu einer erneuten Prüfung derselben auffordern" (Meyer 1870, 364).

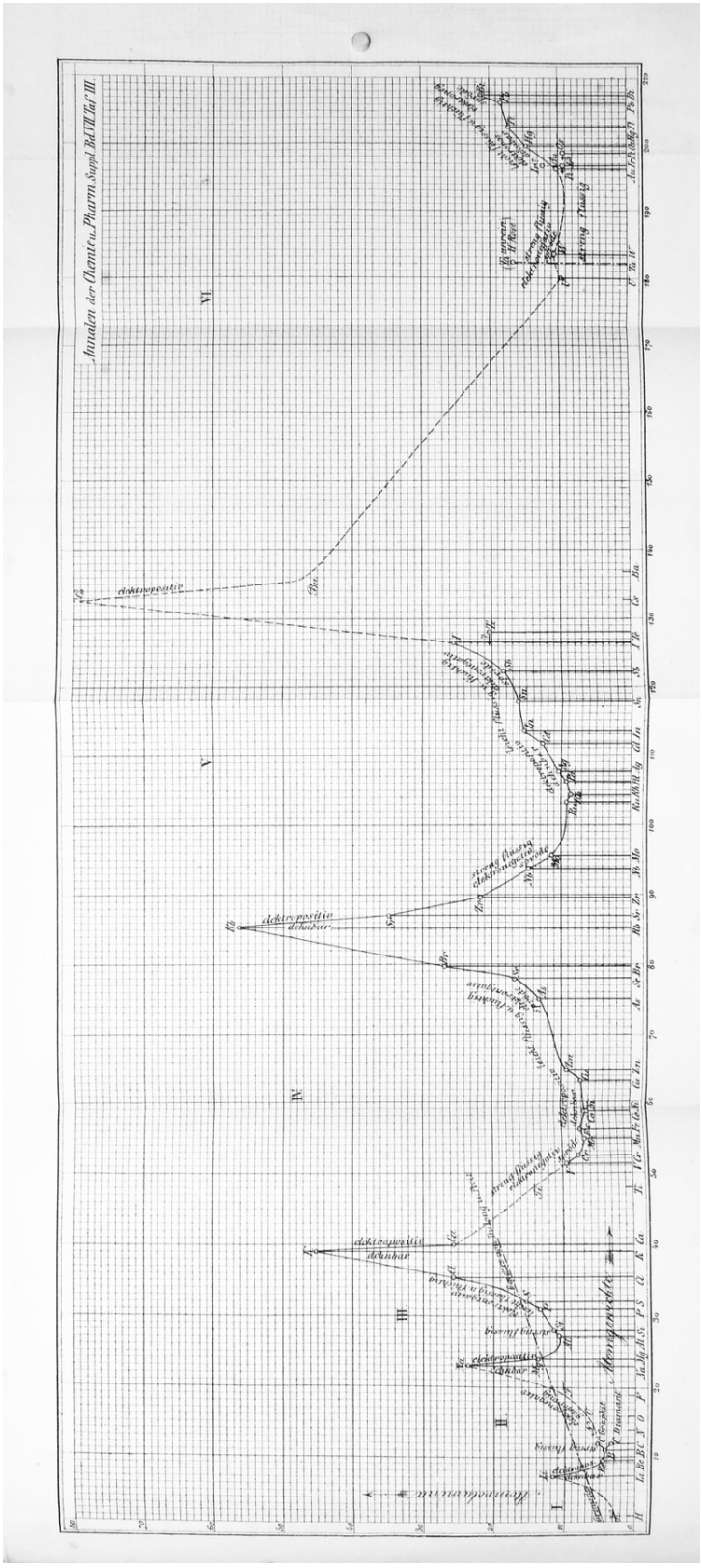


Figure 4.5. Graphic representation of periodicity attached to the end of the volume (Meyer 1870).

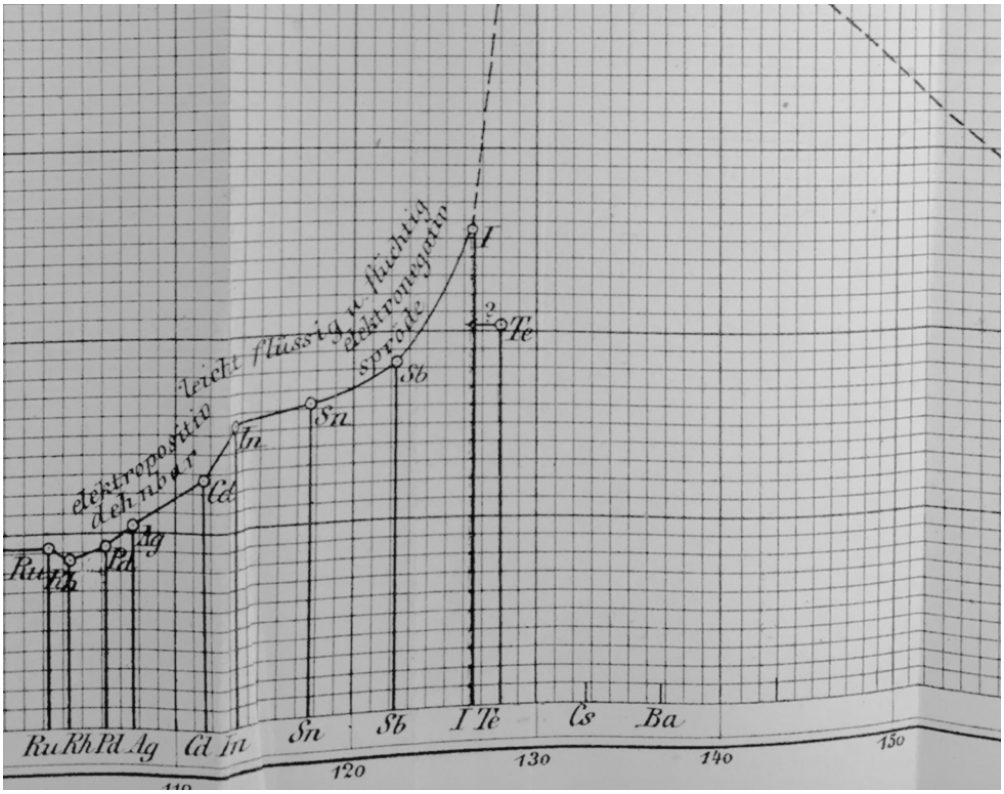


Figure 4.6. Detail of the atomic volume curve (Meyer 1870).

Earlier, I suggested that Meyer's 1864 systems revealed the valuing of carefulness by excluding the less-well-known elements. We also saw that this was the case for the table and curve of 1870. However, the atomic volume curve gives us a new, and perhaps the most striking visual indicator of Meyer's valuing of carefulness. As can be seen from the curve, that the line starts with a broken line, and then changes to a smoother, continuous one. For example, after iodine and tellurium, the continuous line breaks as Meyer chose to express the trend with a discontinuous line (see figure 4.6 for a close-up). The differences in the appearance of the lines drew attention to the differences in the quality of data, which indicates Meyer's valuing of carefulness. In Meyer's words, "[w]here the knowledge of the atomic volume of one or more elements is lacking, the curve is drawn in dotted lines" (1870, 359).

Crucially, Meyer reserved the continuous line for the well-known atomic weights and volumes, whereas the broken line was for the weights and volumes that were not as well

known. The contrasts in different versions of the atomic volume curve then reflect how data on volume and weights of elements became more reliable. For example, if we compare the 1870 version of the curve with its subsequent versions e.g. figure 4.10), it becomes evident that broken areas become continuous. However, interestingly, the earliest version of 1870 is also the smoothest of Meyer's curves, as the later versions (see 4.9 and 4.10) had gaps where Meyer did not think it warranted to indicate a trend at all. For example, in 1888, when commenting on the sections between bismuth and thorium, and cerium and tantalum, Meyer stated that "[a]t the present time we are scarcely in a position to determine whether the curve of atomic volumes will in this case be analogous to the former sections of the curve" (1888, 156).

Before I introduce the later versions of the atomic volume curve, it would be helpful to appreciate that Meyer's utilisation of different kinds of lines for different kinds of data was a technique used by Neumann's other students in the 1850s. The similarities in the employment of different lines are especially striking when we examine the graphs of Heinrich Wild who was Meyer's contemporary at the Königsberg seminar. For this reason, before bringing in Meyer's later versions of the atomic volume curve, I will make a closer comparison between Meyer's curve of 1870 and Wild's 1856 graph. (Apart from Wild, also Oskar Emil Meyer and Georg Herman Quincke used similar techniques, but for my purposes considering only Wild's graph is sufficient).

Drawing the Line: the Graph of Wild

Heinrich Wild took part in the physical division of Neumann's seminar in 1856. After having attended Neumann's lectures on theoretical physics and theory of light, Wild conducted his own independent investigation to determine the intensity of brightness (Olesko 1991, 225). In this investigation, Wild used a photometer (an instrument designed for determining whether two illuminated surfaces were equally bright) and modified it to a polarimeter, which then could be used for determining the intensities of the natural and polarized parts of light (Olesko 1991, 225-227).[31] After conducting his empirical investigation, Wild

[31] According to Olesko, Wild set unusually high expectations for the precision of his instrument at detecting brightness levels, which makes him Neumann's first student to "refer to precision explicitly in terms of decimal places" (1991, 226). For Wild, it was not enough to reach the levels of an exceptional human eye (which could differentiate 1/100 to

represented the optical intensities as a function of angular distance from the sun (Olesko 1991, 229).

As can be seen from the graph (see figure 4.7), Wild used different lines for different data. Wild drew the more uncertain parts of the curve with a dotted line in order to distinguish the data from results he knew to the precision of three decimal places (Olesko 1991, 228). Thus, similarly to Meyer's graphs, Wild's achievement in measuring data precisely came with an unwillingness to combine good data with the more dubious kind (1991, 229). With

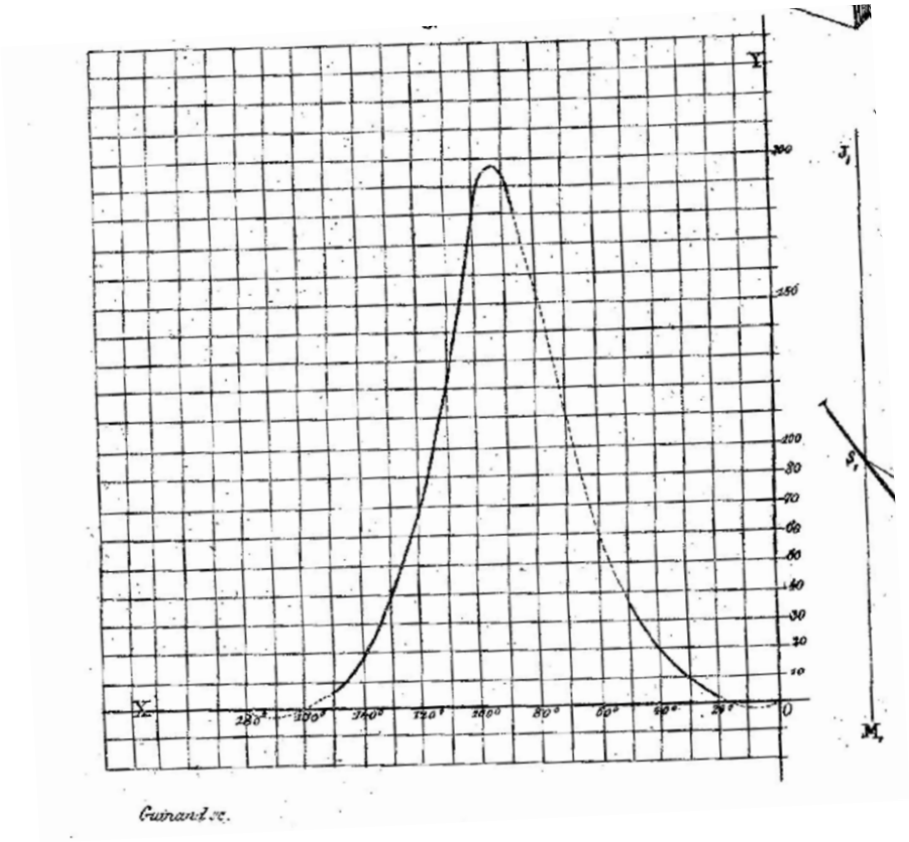


Fig. 4.7. Wild's graph attached at the end of Poggendorff, J.C. 1856. *Annalen Der Physik Und Chemie*. Berlin. Wild's article can be found from the same volume from pp. 235-274.

1/120 difference in brightness). Instead, Wild's aim was to reach to a precision of three decimals, which required the precision of 1/500 to 1/1000 difference in brightness. Wild's ambition for reaching a high level of precision expressed through multiple decimal places forms an obvious similarity between Wild and Meyer. As I noted earlier in this chapter, Meyer was the only chemist engaged in classification of chemical elements in the 1860s who expressed atomic weights in two decimal places.

Meyer's systems, we saw that Meyer refused to place the well-established atomic weight data on the same level with the uncertain kind, and excluded some elements as they were not as thoroughly known. Furthermore, similarly to Meyer's graph of 1870, Wild's graph of 1856 allowed to simultaneously display a generalised result and to distinguish between different levels of certainty (1991, 228).

To sum, Wild's example shows that the main benefit of graphs was to present a generalisation founded in observations and to distinguish effectively between different kinds of data at a single glance.[32] While the error analysis of both Meyers and Wild echoes the sentiment of the Königsberg seminar, it remains unclear whether graphical methods were part of Neumann's pedagogy or an innovation that the students made more independently. Olesko maintains that Wild's application of error analysis to graphical analysis was unusual, and marks a difference from Neumann's favoured tabular form.[33] Thus, while the appetite for distinguishing between more certain and less certain data stems from Neumann's seminar, it is more likely that Meyer picked up graphic methods from Wild, his brother, and other students at the seminar instead of Neumann. After publishing his investigation on brightness, Wild returned to Neumann's seminar at that very semester when Meyer had joined the group of students.

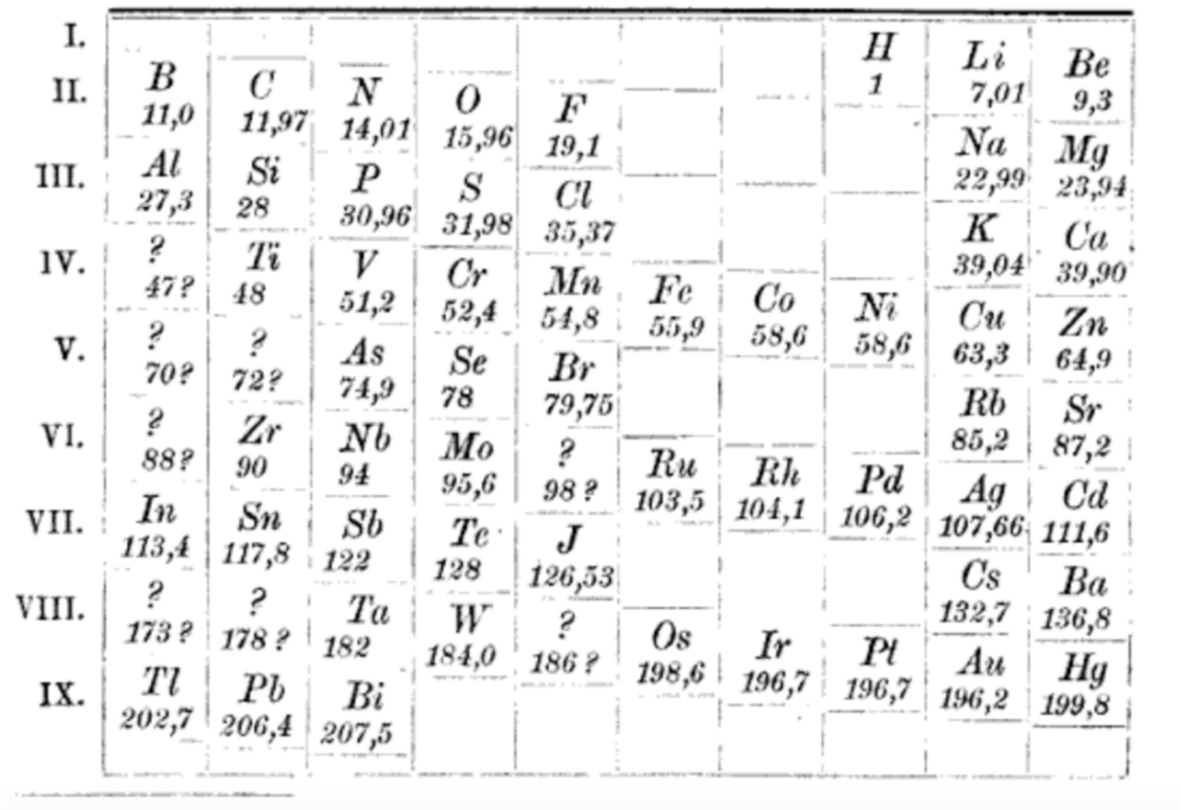
4.5 Updated Atomic Volume Curves and Tabular Systems

Two years after the publication of the atomic volume curve, the second edition of Meyer's textbook was published. The new edition included a more substantial discussion of the periodic dependency between atomic weights and other properties of the elements, where Meyer introduced a new tabular representation (figure 4.8), and a new version of the

[32] Apart from Wild, Julius Lothar Meyer's brother Oskar Emil also utilised different kinds of lines for different kinds of data. When Oskar Emil investigated the viscosity constant of salt solutions as a function of the concentration of each solution, he employed dashes and dots to signify how different measurements were taken from different substances (1991,257). See also Olesko's discussion of Georg Herman Quincke (1991, 241-250).
[33] For Neumann, the tables were a desirable format as they implied the data could be combined "because all sets fell within certain tolerances of error" (1991, 230).

atomic volume curve. In particular, his discussion shows the valuing of both carefulness and completeness in accounting for qualitative properties of the elements.

In Meyer's introduction to the new table, Meyer again brought attention to the numerical differences of 16 between atomic weights of elements with similar valency. He noted that this regularity was probably not accidental and that such relations were formed by elements that fit the six natural families. In order to show the natural groupings, Meyer elaborated how some triplets and pairs of elements were similar, where he only discussed their isomorphism and similar compounds (Meyer 1872, 301). Regarding the valuing of carefulness, Meyer noted that the placement of ruthenium (Ru=193,5) and osmium (Os=198,6) was uncertain. Nevertheless, he placed them in the same vertical column with iron (Fe) (1872, 301).



I.									H 1	Li 7,01	Be 9,3
II.	B 11,0	C 11,97	N 14,01	O 15,96	F 19,1					Na 22,99	Mg 23,94
III.	Al 27,3	Si 28	P 30,96	S 31,98	Cl 35,37					K 39,04	Ca 39,90
IV.	? 47?	Ti 48	V 51,2	Cr 52,4	Mn 54,8	Fe 55,9	Co 58,6	Ni 58,6		Cu 63,3	Zn 64,9
V.	? 70?	? 72?	As 74,9	Se 78	Br 79,75					Rb 85,2	Sr 87,2
VI.	? 88?	Zr 90	Nb 94	Mo 95,6	? 98 ?	Ru 103,5	Rh 104,1	Pd 106,2		Ag 107,66	Cd 111,6
VII.	In 113,4	Sn 117,8	Sb 122	Te 128	J 126,53					Cs 132,7	Ba 136,8
VIII.	? 173 ?	? 178 ?	Ta 182	W 184,0	? 186 ?	Os 198,6	Ir 196,7	Pl 196,7		Au 196,2	Hg 199,8
IX.	Tl 202,7	Pb 206,4	Bi 207,5								

Figure 4.8. Spiral system of 1872 (Meyer 1872, 301).

After these preliminaries, Meyer put forward the spiral system of elements (figure 4.8), where the resembling pairs and triplets mentioned earlier were spread on the vertical columns. The table included 56 elements where Meyer noted that their atomic weights had been reliably determined, which again demonstrates how Meyer brought attention to the quality of observations (1872, 302). Apart from the 56 reliably determined elements, the table included eight vacancies. Meyer fitted the available placements with question marks and interpolations for the atomic weights of the elements that would eventually be placed in the gaps (1872, 301).

Meyer then highlighted how the table could be transformed into a spiral system or a cylinder if the group starting with boron and aluminium was “aligned with those of the alkaline earth metals, beryllium, magnesium, calcium &etc.” (1872, 302). Such arrangement would show that the elements placed on top of each other would depict natural families of elements, where the similarities would be most striking in groups III, V, VII, and IX.

Like with the tabular system of 1870, Meyer stated that achieving such a “regular arrangement” required changing the atomic weights of elements that had nearly equal weights (1872, 302). Thus, tellurium was placed before iodine, osmium before iridium and platinum, where all of these are placed before gold. Like with the earlier investigation of

1870, Meyer stated that the acceptability of changing the atomic weights in this way would be determined by further investigations to atomic weight values (1872, 302). In any case,

> As one runs through the series of elements of the size of the atomic weights, the periodicity of the properties in of their dependence on the size of the atomic weight is very clear. While the differences of the immediately following atomic weights do not seem to obey a simple law, there are quite regular relationships between the atomic weights of the members of one and the same family. (Meyer 1872, 302-03).

As noted in the introduction to this thesis, Meyer’s denial of simple laws forms a stark contrast with Newlands’ rhetoric.

Meyer then proceeded to illustrate the regular relationship through graphic means. In his introduction of the new version of the atomic volume curve, Meyer again emphasised how he had reserved different kinds of lines for different kinds of data. In contrast to the graph of 1870, however, the curve could not be drawn *at all* in some areas. This was because "the density and hence the atomic volume are unknown for a number of elements," so Meyer saw that "this curve cannot be completely drawn" (1872, 306). Where the clues were lacking, Meyer left a gap in the curve (e.g., between barium/Ba and tantalum/Ta see figure 4.9).

After the preliminary introduction of the atomic volume curve, Meyer updated his earlier discussion on the spread of the qualitative properties with a fuller elaboration on the same subject. In particular, Meyer discussed the relationship between atomic weights and

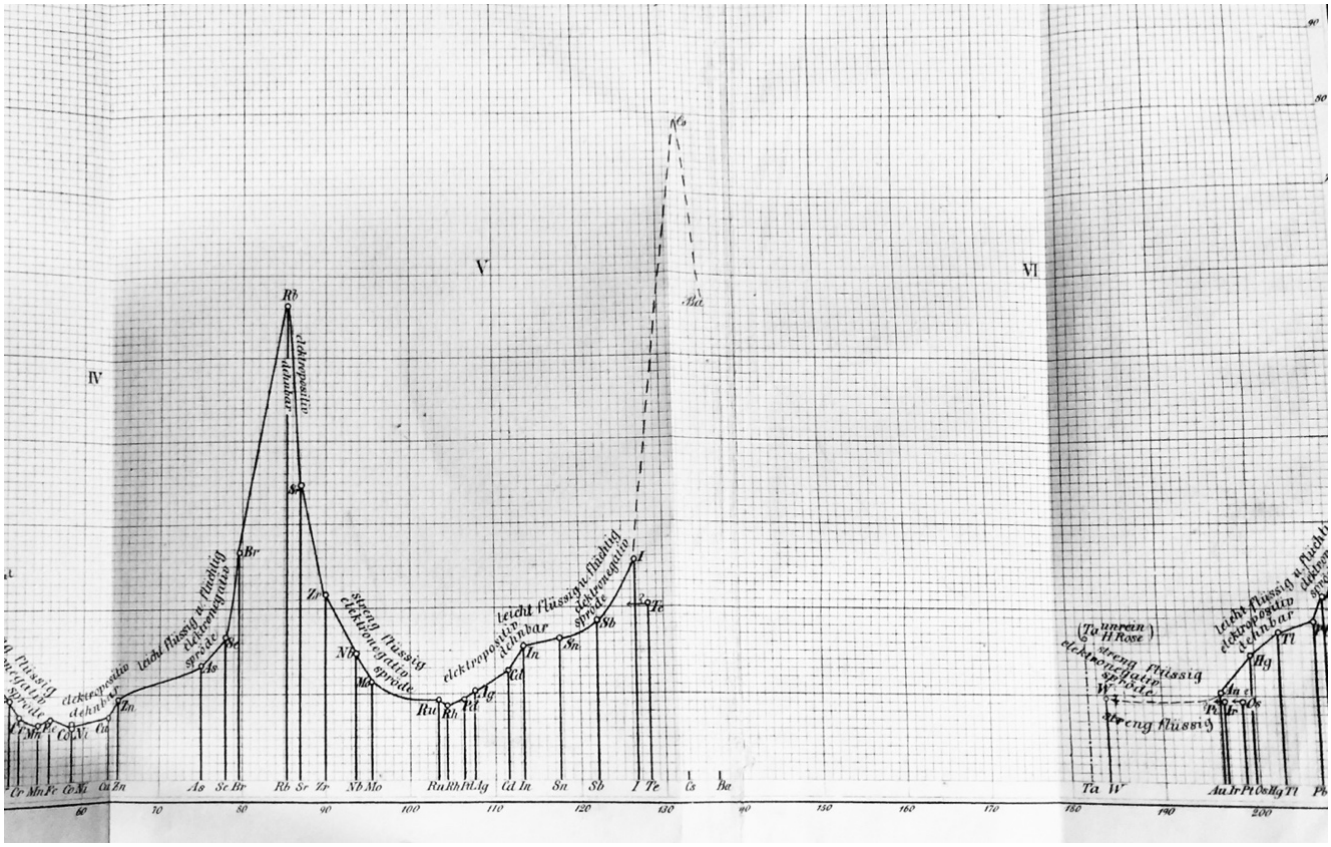


Figure 4.9 Extract from Meyer's table of 1872, attached to the end of the book (Meyer 1872).

volatility, ductility, and malleability (1872, 309-312), electrochemical behaviour (1872, 324-327), and how elements refracted light. Meyer then explained how all these properties were distributed around the minima and maxima of the curve. Similarly to the 1870 version, Meyer also indicated their relationship with atomic weight by writing on the slopes of the curve. This gives us an idea of Meyer's growing valuing of qualitative completeness.

Apart from putting forward a fuller discussion of the qualitative properties that he had already brought up in 1870, Meyer also discussed a variety of previously unmentioned physical and properties of elements. These new additions included Mendeleev's observation of how oxidisation depended on atomic weights, and how the elements formed compounds with chlorine and hydrogen (1872, 328-337). This suggests that alongside valuing of carefulness, Meyer also valued completeness in considering qualities of the elements, albeit his systems were incomplete in the sense that they did not include all the chemical elements.

In this textbook of 1872, Meyer also elaborated on the applications of the periodic systems. While both the tabular system and the atomic volume curve revealed a regularity that allowed Meyer to interpolate the atomic weights for some of the missing elements, Meyer warned that such knowledge was still in stages of "early childhood" and did not warrant drawing too many conclusions (1872, 338). However, the regularity could be useful at identifying errors in experimental results, which Meyer illustrated with caesium (to be discussed in chapter 8). Like in his previous article of 1870, Meyer emphasised that the regularity did not warrant changing the atomic weights without the support of further "stoichiometric determination" (1872, 339).

However, despite this warning against changing empirical results on the basis of theoretical regularity, Meyer also acknowledged the appeal of predicting the properties of undiscovered elements. In particular, Meyer noted that the analogies between the elements suggested that the missing members that would fill the gaps later would show similar properties to other elements on the same row (1872, 343). After having acknowledged this temptation, Meyer also warned that

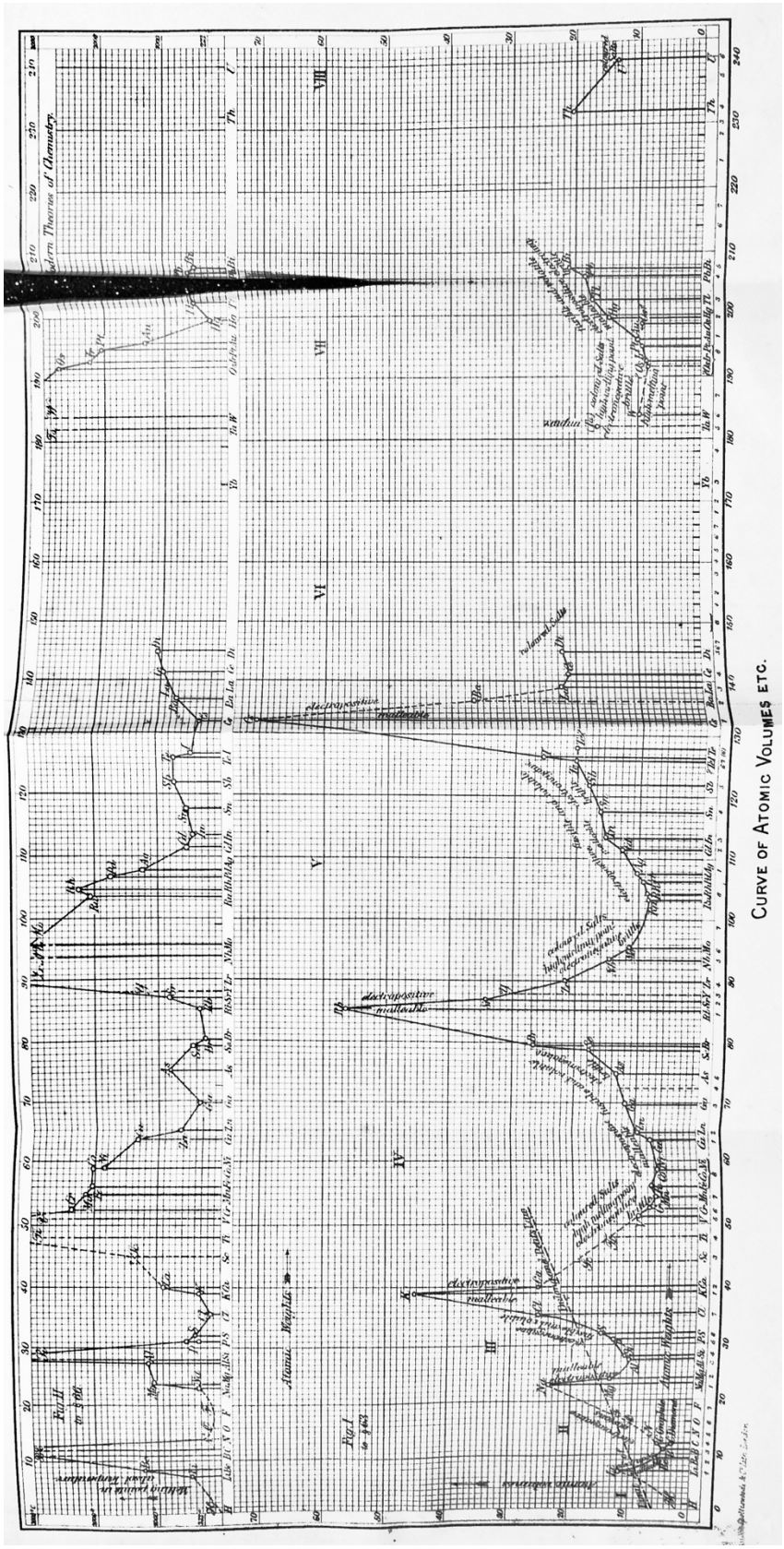


Figure 4.10. Meyer's graph of 1888, attached to the end of the book (Meyer 1888).

> (…) the prediction of the properties of the missing elements is in any case one of the most attractive but also the most difficult tasks of chemical science … But if we are aware of the weakness of our weapons, it will nevertheless be permissible to test our powers by predicting the properties of undiscovered elements with greatest possible probability, in order to later perhaps compare them with the actually observed ones and then be able to judge the value or lack of value of our theoretical speculations. (1872, 344).

This statement was not an encouragement to make predictions, as immediately afterwards Meyer argued that Mendeleev's predictions were arbitrary and risked bringing in error (1872, 345). Meyer's assessment of Mendeleev's predictions shows that at most, Meyer admitted predictions insofar as they were intended to show how theoretical estimates and empirical data supported or differed from one another (as noted originally by Gordin 2012, 72-74). Such a constricted role for prediction was needed because overly bold claims made on the basis of theories could jeopardize Meyer's more general goal: the demonstration of the usefulness of theories for chemical investigations (more on this in chapter 8). False predictions could lead to some serious problems:

> If chemistry is to be spared new deeply distressing catastrophes, one must before all else strive for a correct valuation of hypotheses and theories, that, as we hope, will soon become a general resource for all researchers. As we have happily overcome the false disdain for hypotheses and theories and the overweening fear of their dangerousness, so we must also take care to avoid the opposite extreme in chemistry, the simplistic erection, overvaluation and dogmatization of hypothetical assumptions. (Meyer 1872, 362 translated by Gordin 2012, 73).

In his account of Meyer's balancing between theory and observation, Gordin argues that it is crucial to appreciate that Meyer made the statement in a textbook. The fact that Meyer did not pursue detailed predictions on the properties of undiscovered elements beyond the brief interpolations was a pedagogical lesson on "the conjunction of observation and theory … a point to be drilled to students" (Gordin 2012, 74).

It is especially telling that Meyer did not change his position on predictions *after* the discovery of the elements that matched up to Mendeleev's predictions. For example, in the

1888 English language translation of the 5th edition of his textbook, Meyer stated that "[t]he relations between the atomic weights and the properties of the elements, though useful, are still insufficient aids for the determinations of the atomic weights. It can be stated generally what atomic weights an element cannot have; but we can only make conjectures as to which is the real atomic weight of an element" (1888, 159-160). This was because "based upon these relations, very different conclusions have been drawn as to the atomic weight of one and the same element" (1888, 160).

Meyer's later research interests dovetail with his suggestions about the relationship between theory and observation. After publishing the second edition of the textbook, Meyer's chief work consisted in redetermination of atomic weights. From 1878 to 1895, Meyer engaged in this line of research together with Seubert, co-authoring several papers and a book on the subject (Meyer and Seubert 1883; see also Boeck 2015, 48).

4.6 Conclusion: The Indicators of Carefulness

So far, I have outlined how Meyer developed his periodic systems in 1862-1872. In the course of my discussion, I brought attention to how Meyer emphasised the regularity expressed by his systems, but also warned against assuming any simple underlying laws, which forms a contrast with Newlands' approach. We also saw that Meyer's systems show the valuing of completeness in accounting for the qualities of chemical elements. This was especially the case for the article of 1870 and the second edition of the textbook, which demonstrated that Meyer sought to bring in a variety of qualitative properties in relation to his systematisation. However, what makes Meyer's systematisation distinctive is his signalling on the quality of observations that eventually gave rise to the periodic system. As noted in the introduction to this chapter, I suggest interpreting Meyer's strategies for indicating the quality of observations as valuing of carefulness, where I define carefulness as

> *signalling differences in the quality of observations of the phenomena in the problem area, and the prioritization of good quality observations over poorer ones.*

In chapter 6 of this thesis, I offer a more detailed argument in favour of the above understanding of carefulness. In this concluding section, I will briefly bring together the

instances of carefulness that were scattered across the previous sections. The following table summarises all of the features of the systems that speak for the valuing of carefulness during their construction:

1.	2.	3.
Expressing atomic weights in an exact form.	Making uncertainties explicit.	Unwillingness to combine precise data with imprecise data.

The first indicator concerns Meyer's unusual choice of expressing the atomic weight in a highly exact form. We saw that in the very first system of 1864, some of the elements in the system were designated by two decimal places. Meyer continued to express atomic weights to this degree of precision throughout all of the systems, albeit not all of the atomic weights were expressed to such a degree of precision. The choice of two decimal places is significant because there was a greater tendency to round off numbers than to express atomic weights with several decimal places (van Spronsen 1969, 112).

The second indicator of Meyer's carefulness in developing his periodic systems is his tendency to make uncertainties explicit. For example, in the first tables of 1864, Meyer brought attention to cases where atomic weights were doubtful. Furthermore, we saw that in the table of 1868 Meyer made the tentativeness of aluminium's placing explicit by situating it to two different locations. With the atomic volume curve of 1870, we saw that Meyer employed the dotted line to express atomic weights and volumes that were not very well established. In 1872, Meyer refrained from drawing the line for those elements that he regarded as not sufficiently well known.

The third indicator of carefulness captures Meyers's exclusion of some chemical elements from the system. For example, we saw that in 1864, Meyer chose to focus on elements

that were well-characterised, and in 1870, he excluded nine elements on the grounds that they were not very well known (exclusion of hydrogen is not taken into account here). The significance of Meyer's omission is best appreciated when we compare the number of elements in Meyer's system with the number of elements in the systems of other chemists. We saw that Meyer's earliest table alone had 28 elements. In conjunction with the two smaller tables they cover 50.[34] By 1868, the number of elements rose to 53, and by 1870, we can find 55 on the tabular system. Both the spiral table and graph of 1872 included 56 elements. In isolation, the gradual raise in the number of elements included might not seem that telling, but if we compare these numbers to Newlands's 62 elements in 1865, William Odling's 57 elements in 1864, and Mendeleev's 63 in 1869, it becomes clear that Meyer was curating the elements to the tables with more care than his contemporaries.

[34] The missing elements include at least terbium, erbium, lanthanum, thorium and aluminium, all of which had been discovered recently.

Chapter 5. Completeness of Mendeleev's Periodic System

5.1 Introduction

Dmitrii Ivanovich Mendeleev (1834-1907) was aware that his contributions to chemistry would be remembered. We get a sense of Mendeleev's attempts to control his image from his account of the course of his life, which he issued a year before his death. In this outline, the more distant past appears patchier than Mendeleev's description of the more recent events. We hear that he was born in the Siberian town of Tobol'sk (Mendeleev 1951, 13; see also Pribyl'skij, Valitov, and Zagorodnyuk 2009). His father, a former headmaster of a local gymnasium, was already blind at the time of Dmitrii's birth (Mendeleev 1951, 13). In 1850, three years after the death of his father, Mendeleev travelled to Moscow together with his mother, his sister Liza, and servant Yakov to enroll in Moscow University (Mendeleev 1951, 14). He was not admitted, so they moved westwards to St. Petersburg, where Dmitrii eventually started his studies at the Main Pedagogical Institute of St. Petersburg – an institution where his father had studied (Mendeleev 1951, 14). Soon after their arrival to St.Petersburg, Liza and Dmitrii Mendeleev lost their mother, and he fell ill (Mendeleev 1951, 14).

It is fitting that a distinctly *pedagogical* problem would give rise to the major discovery of a chemist who happened to be a son of a headmaster and attend a pedagogical institute as an undergraduate student. However, before Mendeleev the teacher, there was Mendeleev the student, and the outline of his life subtly emphasises that he was an ardent one, too. When the young, orphaned Mendeleev was confined to hospitals because of his poor health, he used the time for studying (Mendeleev 1951, 15). His undergraduate efforts culminated in two pieces of work: a published article reporting a chemical analysis of Finnish minerals, and an undergraduate thesis which looked into isomorphism in minerals (Kaji 2018, 221). As noted by the historian Masanori Kaji, who has given accounts of the conceptual and social origins of the periodic law, the central theme of Mendeleev's thesis (the relationship between crystal form and composition) directed Mendeleev's attention to the similarities of substances. Kaji regards this as the beginning of Mendeleev's interest in classification of substances (2018, 221).

After his undergraduate studies, Mendeleev became a teacher at a gymnasium in southern Russia. However, his stay was not very long. Already in 1856, Mendeleev returned back to St. Petersburg to defend his master's thesis on specific volumes. For the thesis, Mendeleev adopted Avogadro's hypothesis (which he called Gerhardt's law) and Gerhardt and Laurent's system of atomic weights (Kaji 2018, 222).

Many Russian scholars spent time in the German states and followed their model in establishing institutions and in running journals (Gordin 2009, 40). Mendeleev was one of the many early-career scholars who travelled to Germany to pursue his post-doctoral studies. Like Meyer, Mendeleev lived in Heidelberg, where he worked with Bunsen. The geographic location allowed him to attend the first International Chemical Congress in Karlsruhe (Gordin 2009; DeMilt 1951). After having read Cannizzarro's pamphlet on standardising atomic weights, Mendeleev reported its contents to his teacher A.A. Voskresenskii (1808-1883), and the letter was published later in a St. Petersburg paper *St.-Peterburgskie Vedemosti* (DeMilt 1951).

Upon his return to St. Petersburg, Mendeleev started writing textbooks on chemistry. Although Mendeleev is most well-known for his textbook on inorganic chemistry, his first book was on organic chemistry. There, Mendeleev proposed "the theory of limits" for classifying organic compounds based on their substitution reactions and their degree of saturation (Kaji 2018, 223). The theory was quickly brushed aside, but the textbook was well received in Russia and was awarded a prize from St. Petersburg Academy of Sciences (Kaji 2018, 223). After finishing his first textbook, Mendeleev intended to write another one on inorganic chemistry, but he put that plan on hold. Meanwhile, Mendeleev continued developing the themes of his previous dissertation on silicates. He named the substances with constant physical properties and varied compositions "indefinite compounds," where his interest in them led him to be suspicious of atomic theory (Kaji 2018, 223). As we will see in the next sections, his project of systematisation of the elements furthered his earlier interests in classification, where Mendeleev's suspicion on atomic theory was something that characterised his approach to the systematisation of the elements.

In 1864, Mendeleev he was appointed a professor of St. Petersburg Technological Institute. He remained at the post until 1867, when he replaced his mentor Voskresenskii as the extraordinary professor at St. Petersburg University. In this post, Mendeleev started writing his textbook on inorganic chemistry. It was in this setting of writing *Principles of Chemistry* (*Osnovy khimii*, published in two volumes in 1869 and 1871) that Mendeleev discovered the earliest version of the classification of the chemical elements that would become known as the periodic system (Gordin 2004, 24; Rawson 1974, 192-196). The need for a classification arose when Mendeleev was searching for a way to organise the second volume of the textbook (Gordin 2004, 24). The first volume Mendeleev structured around chemical practices, where it focussed on providing "natural-historical information" of chemical substances, including various definitions and plans for experiments (Gordin 2004, 24). The first textbook was concluded with a discussion of sodium, chlorine, and the halogen family, which left over fifty elements for the second volume. Squeezing such a high number of elements to a single volume was a formidable task, and forced Mendeleev to consider between different organization methods for the remaining elements.

Apart from the challenge of finding a place for the remaining elements, another crucial difference between the first and second volumes of Mendeleev's textbook is his distinction between simple substances and elements. Gordin characterizes the distinction as follows:

> There is, strictly speaking, no such thing as an element in nature; what exist instead are "simple substances," a concept initially developed by Antoine Lavoisier. That is to say, no one (even after the advent of scanning-tunneling microscopes) has ever seen "carbon"; instead, they have seen diamond, or graphite, or other forms (and, today, carbon atoms). Oxygen is observable in nature as the oxygen molecule or ozone. We infer the notion of an "element" as the metaphysical basis that relates the various forms, much as Mendeleev later inferred the periodic law as the metaphysical basis to explain the diversity of "elements." (Gordin 2004, 25, emphasis original).

In early February 1869, when writing the first two chapters for the second volume of the textbook, Mendeleev listed the elements according to their weight (Gordin 2004, 26). By the time Mendeleev was writing chapter 4, he was convinced that the best way to organize the elements was to establish a system based on the atomic weights. He started the chapter by discussing the arithmetical differences between the rows, where he drew attention to how they seemed to follow a similar pattern in the halogen group, alkali metals, and alkaline earths (Gordin 2004, 26-27).

Gordin notes that the difficulty of reconstructing Mendeleev's arrival at the idea of using atomic weights as an organizing principle stems from the fact that Mendeleev kept most of his documents and drafts only *after* he believed that he would become famous (Gordin 2004, 27). We only have four papers on the periodic system which precede the first publication on the periodic law (some reproduced in Kedrov 1958a). Despite the scarcity of documents, Gordin has identified at least three ways in which Mendeleev may have come up with the idea. The first option is that Mendeleev listed the elements by order of atomic weights. The second option is that he put together the acknowledged groups (e.g., alkali metals and the halogens) and noticed a pattern in their weights (Gordin 2004, 27). Gordin himself advocates the third option, which can be described as the compromise between the two. In April 1869, Mendeleev described having gathered together bodies with low atomic weight and organised them according to increasing atomic weight. Mendeleev saw these elements to be typical (Gordin 2004, 27). This grouping suggests for Gordin that Mendeleev created the rows and groups simultaneously:

> He took the lightest elements and listed them by rising atomic weight, building a row; but each of the "typical elements" in the top row encoded as typical the properties of the elements below it …These elements stood in for their groups, and Mendeleev could see both patterns at once. (Gordin 2004, 28).

It seems likely that Mendeleev came up with the idea sometime in early 1869, possibly during the process of writing the first two chapters of the textbook. While the system was created in a pedagogic context of writing a textbook, it soon took on a life outside its pages. Mendeleev chiefly developed the system in a series of articles that were published

in various journals in 1869-1871. In the rest of this chapter, I focus on Mendeleev's process of developing the systems in these publications. While many commentators on Mendeleev's periodic system have drawn attention to the completeness of his systematisation (e.g., Brooks 2002, 129; Scerri 2007, 123; Gordin 2018, 11), there has been no substantial analysis of what completeness consists in. I will provide a more thorough analysis of completeness in chapter 6, as such analyses can only be provided after presenting the historical evidence on how Mendeleev valued completeness. For this reason, in the following sections of this chapter, my chief aim is to show how Mendeleev's systems were complete, where I endorse the following definition of completeness:

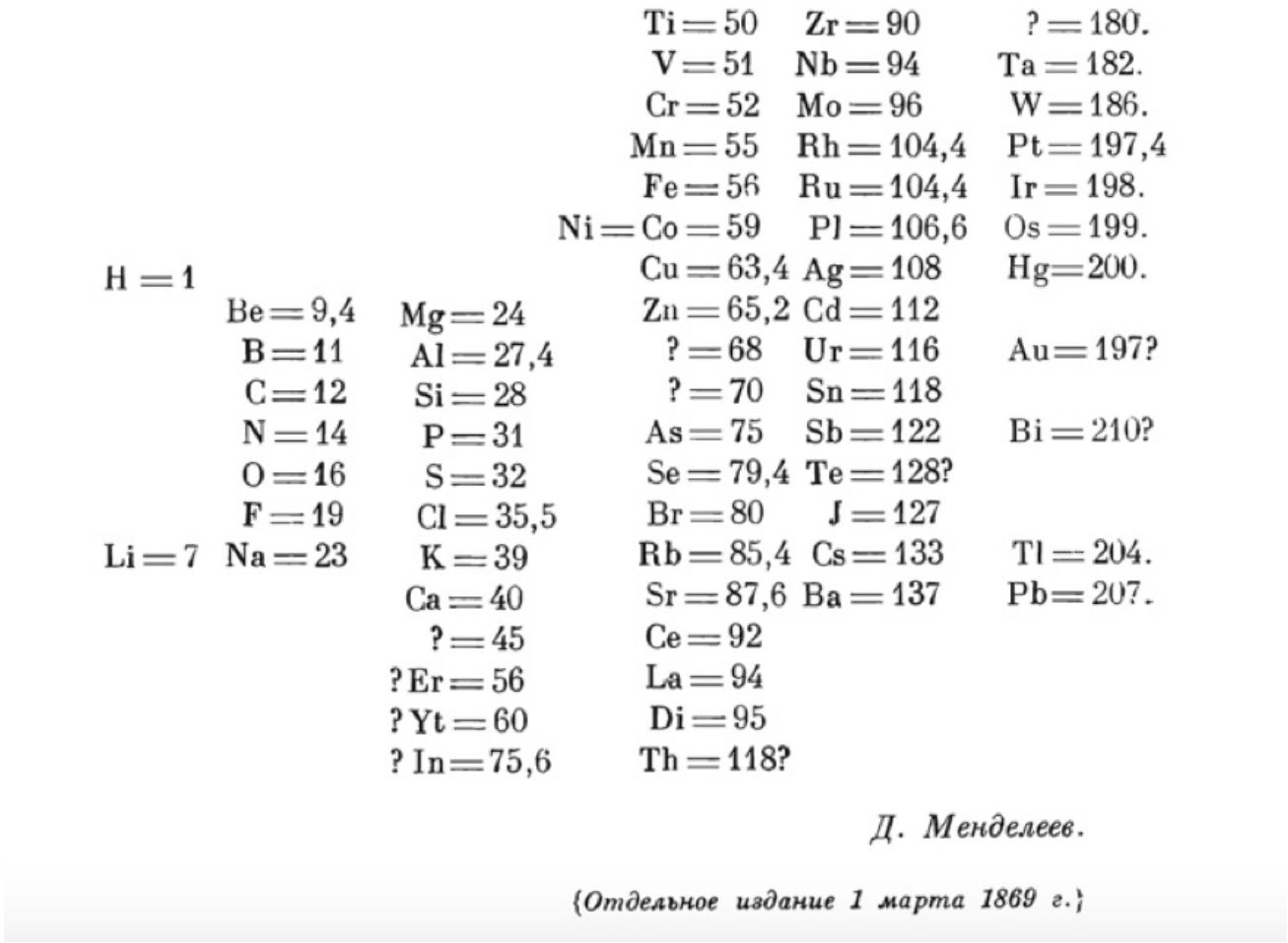
> *Completeness involves wanting to account for as many phenomena (and their observed aspects) as possible in a given problem area, giving special attention to phenomena that are deemed most relevant, and saying something about accommodating phenomena that are likely to enter the problem area.*

I will argue that especially telling indicators of the systems' completeness are (i) the large number of chemical elements incorporated to the arrangement, and (ii) Mendeleev's emphasis on how various qualitative properties and similarities are housed in his systematisation. Although completeness was not the only quality that Mendeleev called for, I will argue that completeness was a crucial attribute for Mendeleev in showing that he had created a natural classification instead of an artificial one. For achieving that goal, the system had to be as complete as possible.

5.2 Mendeleev's First Systems: From an "Attempt" to Correcting Atomic Weights

In contrast to Mendeleev's voluminous discussions of systematisation of 1871, his first publication on the classification of the elements was just a print-out of the system (see figure 5.1). The system was published on 17 February 1869 in the newly-founded Proceedings of Russian Chemical Society (Mendeleev 1869a, 9). The title reveals that was not yet the periodic law of 1871, but "An *Attempt* Systematising the Elements Based on Their Atomic Weight and Chemical Properties" (Mendeleev 1869a, 9, emphasis added).

As can be seen in figure 5.1, the system does not bear a striking similarity with the modern periodic systems. However, as noted by Gordin, the resemblance between modern tables and the first “attempt” can be appreciated when we rotate the image clockwise 90° and reflect it. If we then place the halogen family (F, Cl, Br, J) at the opposite extreme from alkali metals (Li, Na, K, Rb, Cs, Tl), we get a system that appears more similar to the modern ones (Gordin 2004, 29).



			Ti = 50	Zr = 90	? = 180.
			V = 51	Nb = 94	Ta = 182.
			Cr = 52	Mo = 96	W = 186.
			Mn = 55	Rh = 104,4	Pt = 197,4
			Fe = 56	Ru = 104,4	Ir = 198.
			Ni = Co = 59	Pl = 106,6	Os = 199.
H = 1			Cu = 63,4	Ag = 108	Hg = 200.
	Be = 9,4	Mg = 24	Zn = 65,2	Cd = 112	
	B = 11	Al = 27,4	? = 68	Ur = 116	Au = 197?
	C = 12	Si = 28	? = 70	Sn = 118	
	N = 14	P = 31	As = 75	Sb = 122	Bi = 210?
	O = 16	S = 32	Se = 79,4	Te = 128?	
	F = 19	Cl = 35,5	Br = 80	J = 127	
Li = 7	Na = 23	K = 39	Rb = 85,4	Cs = 133	Tl = 204.
		Ca = 40	Sr = 87,6	Ba = 137	Pb = 207.
		? = 45	Ce = 92		
		? Er = 56	La = 94		
		? Yt = 60	Di = 95		
		? In = 75,6	Th = 118?		

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Figure 5.1 Mendeleev’s first system of elements (Mendeleev 1869a, 9).

Apart from the linear atomic weight ordering and expression of the qualitative similarities, it is especially striking that Mendeleev's first published system included altogether 63 elements. The high number of elements forms a similarity with Newlands' systems of 1865 and 1866 but is in contrast with Meyer's systems (and the first attempts of Newlands). Although the high number of elements on Mendeleev's first system is partially explained by the fact that he published it later than Newlands and Meyer, and thus benefitted from having more data on the elements, the lateness of the occasion does not thoroughly explain the contrast; after all, Meyer's system of 1870 included no more than 55 elements. Thus, Mendeleev's inclusion of 63 elements gives the first clue of his aim towards completeness.

There are also some notable similarities between Mendeleev's first system and the systems of Newlands and Meyer. For starters, like Meyer, Mendeleev had added question marks to six elements (Er, Yt, In, Th, Te, Au), which shows that he too distinguished between more tentative and well-established placings. Similarly to Newlands' and Meyer's systems, Mendeleev's "attempt" included four interpolations of the atomic weights of undiscovered elements. If we compare Mendeleev's first system and Law of Octaves, it is evident that Mendeleev too added two elements (Ni and Co) into the same vacancy in 59. However, contrary to Newlands, Mendeleev also placed rhodium (Rh) and rubidium (Ru) in different slots despite their close atomic weights. This suggests that Mendeleev wanted to highlight how nickel (Ni), cobalt (Co), palladium (Pl), and iridium (Ir) belonged to the same natural group, whereas rhodium and rubidium did not. Thus, while it might appear that Mendeleev followed Newlands's approach of prioritising linearity, the placing implies that qualitative similarities guided Mendeleev's organisation more.

Mendeleev's next article, "On the Correlation of the Properties and Atomic Weights of the Elements" offered the first thorough elaboration on the "attempt." The paper was published on the 6th of March 1869, only a few weeks after the first systematisation (Mendeleev 1869b, 10-31). Before introducing his arrangement in detail, however, Mendeleev evaluated other systematisations on the market. In particular, he brought attention to the merits and problems of systems that distinguished between metals and metalloids; acidity

and basicity; the relationship to oxygen and hydrogen; between atomicity, and electrochemical behaviour (1869b, 10-16). In the course of this evaluation, Mendeleev introduced a variety of qualities to describe the attributes of a good systematisation. In particular, he called for rigorousness (*strogost'*), naturalness (*estestvennost'*), completeness (*polnost'*), and systematicity (*sistematichestnost'*), where he especially emphasised the importance of naturalness and systematicity (1869b, 10-15).

While Mendeleev's emphasis on naturalness and systematicity suggests that both could be interpreted as valuable qualities for Mendeleev, I suggest it would be more appropriate to view them as Mendeleev's ultimate aims than valued qualities of his project. This is because Mendeleev invoked other qualities as evidence for having established a truly natural system. In what follows, I will show how completeness and rigorousness served this role, where I suggest that completeness furthered Mendeleev's general goal of having a *natural* system, whereas rigorousness was more tied to systematicity.

In his discussion of the previous systematisation attempts, Mendeleev first brought up the term 'completeness' (*polnost'*) while he was evaluating the groupings of Peter Kremers (1869b, 15).[35] After Mendeleev listed Kremers' grouping of dissimilar elements with triadic relationships, Mendeleev argued that "[b]ut relations of this kind have represented and continue to represent in our minds only some fragmented findings that do not lead to a *complete system* of elements" (1869b, 15, emphasis added). Mendeleev's problem seems to have been that although Kremers' findings allowed for adding a few more elements to natural groups, they did not succeed in bringing together all of the elements. In a similar vein, immediately after this assessment, Mendeleev noted that while Lenssen's, Pettenkofer's, and Sokolov's attempts established some numerical relations between the elements, they failed at "systematic allocation of *all* of the known elements" where Mendeleev's reference to the number of elements can be taken as another nod towards the importance of completeness (1869b, 15, emphasis added).

The importance that Mendeleev attached to completeness also comes clear in his introduction to the first edition of his textbook:

[35] See Boeck (2015) for a description on Kremer's triadic systems.

> In natural sciences there are no axioms that could help in presentation of those sciences like geometry… It is this side of the subject that forced me to add… a more specialised goal: to state together with conclusions a description of the methods of their extraction, enter to one systematic whole *as much data as possible without surrendering to extremeness of a scientific encyclopaedia.* (Mendeleev 1871d, 54, emphasis added).[36]

Mendeleev's statements concerning his goal of systematising as much data as possible show that completeness alone did not pave the way for a systematisation. Avoiding a merely complete list of findings required a rigorous organising principle. This becomes especially clear in his first article on the periodic system. After evaluating Kremers' system, Mendeleev noted that systematising rubidium, thallium, and caesium would not be possible until the identification of a suitable "hypothetical finding that is capable of serving as the support for a rigorous system" (1869b, 15). For Mendeleev, only atomic weights could provide firm enough grounds for such systematisation, because they remained more constant than other properties of elements. After all, they provided the "quantitative data" that was "intrinsic to the element," and was associated both with the individual simple bodies and their compounds (1869b, 17).

To attain such rigor, Mendeleev pursued a system that was organised according to the magnitude of the atomic weights (1869b, 18). However, even though atomic weights allowed for a level of rigorousness, it was not enough just to map some numerical relationships between the atomic weights as Pettenkofer, Lenssen, Sokolov, and others had done. Instead, one had to bring together as many findings as possible. To have a systematisation, it was not enough to have a complete list of findings or a rigorous arrangement that lacked in material, but both completeness and rigor were needed.

[36] Kaji and Brooks note that this preface was completed in 1st of March 1869, so it is likely that Mendeleev wrote the first article elaborating on the system and the above quote around the same time (Kaji and Brooks 2015, 17).

Thankfully, the two attributes of completeness and rigorousness turned out to be mutually inclusive. In outlining his first attempts towards systematisation, Mendeleev described taking the simple bodies with the smallest atomic weights and organising them according to their weight. He then noticed that there is "almost like a period of properties of simple bodies, where even the atomicity [valency] of the elements follows one another in arithmetic order according to their atomic weights" (1869b, 18). After lining up the light elements in this manner, Mendeleev made two similar rows of elements with high atomic weights. It appeared that the row of light elements (Li, Na, K, Ag) was analogous to the rows of heavier ones (C, Si, Ti, Sn, and N, P, V, and Sb). The analogous relationship was so striking that "[i]mmediately a hypothesis was born: are not the properties of the elements expressed by their atomic weights, [and] wouldn't it be possible to form a system based on them?" (1869b, 18).

For Mendeleev, it appeared especially commendable that the atomic weight ordering was complementary with the more traditional ordering according to the "natural groups" or qualitative similarities (1869b, 19-20). The statement of the importance of the natural groups marks the first steps towards Mendeleev's emphasis that completeness was not just about accounting for many elements. Instead, as we will see, completeness involved also accounting for many of their qualitative properties and resemblances. The greater emphasis on what I call "qualitative completeness" marks Mendeleev's systems as different from those of Meyer and Newlands. Although Meyer, too, approached this ideal in his later tables, it will become clear that Mendeleev emphasised it to a greater extent than Meyer did.

In Mendeleev's elaboration on the natural groups, he divided the qualitative characteristics of elements into physical and chemical properties. The former properties referred to physical appearance (e.g., being metallic) and the latter to features such as affinity, basicity, and acidity amongst others. Mendeleev explained that he had noticed a complementarity between displaying both the qualitative data on physical and chemical properties, and the quantitative atomic weight data when he organised the elements in a manner depicted in figure 5.2:

	Ca = 40	Sr = 87,6	Ba = 137
Na = 23	Ka = 39	Rb = 85,4	Cs = 133
F = 19	Cl = 35,5	Br = 80	J = 127
O = 16	S = 32	Se = 79,4	Te = 128
N = 14	P = 31	As = 75	Sb = 122
C = 12	Si = 28	—	Sn = 118

Figure 5.2. (Mendeleev 1869b, 20).

For Mendeleev, the above arrangement clearly demonstrated “some exact relationship” between atomic weights and the natural properties of the elements, or the combining power of the elements (1869b, 20). In particular, Mendeleev drew attention to how “rigorous succession in the change of atomic weight on horizontal rows and vertical column” where only the atomic weight of tellurium disrupted the ordering (Mendeleev suspected it had been inaccurately determined) (1869b, 20). Furthermore, Mendeleev noted how the elements in the group of fluorine (F) combined with one unit of hydrogen; elements in the group of oxygen (O) with two units of hydrogen; elements in the group of nitrogen (N) with three, and carbon with four hydrogen and chlorine units (1869b, 20). For Mendeleev, the gradualness in combining with hydrogen and chlorine demonstrated that the atomic weight ordering did not disrupt the natural groups, but supported displaying them (1869b, 20-21). As we will see, he continued emphasising the importance of such gradualness, because it helped him to demonstrate that he had succeeded in establishing a natural system of elements.

After the introduction of the preliminary grouping depicted in figure 5.2, Mendeleev proceeded to the fuller classification that he had published previously (see figure 4.1). In his elaboration of the system, Mendeleev first considered some similar elements that his ordering had placed afar. For completeness, this discussion is informative as it shows that accounting for all the qualitative features was far from straightforward. As there were several qualitative similarities that the system could encode, Mendeleev had to choose which ones to express and which ones to brush aside. After having discussed some alternative ways of grouping the elements, he concluded that it was best to adhere to the organisation presented in figure 5.1. This was because it grouped elements that had more

essential similarities or more similarities between one another than the alternative groupings (1869b, 26-27).

The remainder of Mendeleev's discussion concerned primarily the gaps left in the system, which he suggested would be filled with undiscovered elements. Interestingly, Mendeleev anticipated that the elements with atomic weights close to hydrogen would be included, albeit his later predictions of 1870-71 did not concern elements with such light weight (Mendeleev's 1904 prediction of coronium and ether alluded to this comment of 1869). Mendeleev concluded his discussion with eight remarks which were published later in German together with the tabular system (Scerri 2007, 303). The first remark directed attention to the periodic change in the properties of the elements, and four others highlighted the chemical properties spread on the system (1869b, 30). In sum, this first substantial article demonstrated that although atomic weights were important for a rigorous system, there were in fact two organisation parameters: atomic weights and qualitative similarities.

Earlier, I noted how Mendeleev emphasised that the gradualness in combining with hydrogen and chlorine was compatible with expressing natural groups. Mendeleev's discussion gave us one of the first hints of how completeness was not just about accounting for a large number of elements, but concerned accounting for qualitative properties of elements, which in this case concerned the combining power with chlorine and hydrogen. The importance of accounting for qualitative properties – which I suggest is part and parcel of valuing of completeness – becomes especially clear from Mendeleev's next article, which primarily focussed on specific volumes (Mendeleev 1870a, 32-49).

Before bringing in the specific volumes of elements, Mendeleev started this article (reportedly written in August 1869 but published in 1870) with a discussion on qualitative similarities of the elements more broadly. To demonstrate how his system conveyed qualitative similarities, Mendeleev first separated chemically similar elements into two categories: those with different atomic weights, and those having close atomic weights (1870a, 32). In his consideration of the first category – the resembling elements with different atomic weights – Mendeleev suggested that the elements could be organised

according to the magnitude of their atomic weights to groups which “clearly demonstrate the periodic dependency of properties on the atomic weights” (1870a, 32) (see figure 5.3).

If we compare this arrangement with the system presented on figure 5.1, it is visible that Mendeleev has turned it 90 degrees to left, so that horizontal rows are now vertical columns. Mendeleev highlighted how the first column (the alkali group) provided a good example of how elements with different atomic weights resembled each other (1870a, 33). In particular, the members formed isomorphous compounds; there was a gradualness in their steps of oxidization; distinguished between metals and metalloids; accounted for valency (which Mendeleev called atomicity); brought together similar elements, and kept dissimilar ones apart; and, finally, clarified homologues (1870a, 33-34). This first column of elements – which later became the first vertical column in Mendeleev’s next version of the system – shows how he both took into account chemical properties in the organisation of the system and, crucially, articulated how the chemical properties supported the systematisation. This forms a contrast especially with Newlands’ approach, where Newlands only mentioned that similar elements could be found in the system.

After articulating the similarities between elements with different atomic weights, Mendeleev considered similar elements with close atomic weights. In particular, Mendeleev discussed four well-known groups – cerite metals, palladium metals, platinum

Li = 7	Be = 9,4	B = 11	C = 12	N = 14	O = 16	F = 9
Na = 23	Mg = 24	Al = 27.4	Si = 28	P = 31	S = 32	Cl = 35.5
K = 39	Ca = 40	—	—	—	—	—
Cu = 63,4	Zn = 65,2	—	—	As = 75	Se = 79,4	Br = 80
Rb = 85,4	Sr = 87,6	—	—	—	—	—
Ag = 108	Cd = 112	—	Sn = 118	Sb = 122	Te = 128?	J = 127
Cs = 133	Ba = 137	—	—	—	—	—

Figure 5.3 (Mendeleev 1870a, 32.)

group and iron group, where “[t]hese groups of metals have in the abovementioned system completely distinct positions according to the size of their atomic weight and partly according to the chemical properties shown by their compounds” (1870a, 34). The similarities were demonstrated in the way they oxidised and provided compounds with ammonia, which revealed that ordering according to the atomic weights also allowed the display of natural groups.

This treatment of the similarities between elements with various weights was only an introduction to the main topic of the article: the relationship between the physical properties of atomic volumes and specific weights (1870a, 36). Mendeleev’s discussion of the dependency of atomic volume on specific weights is complex and lengthy, but for showing that completeness was about accounting for many chemical and physical qualities, it suffices to say that Mendeleev identified a similar relationship as Lothar Meyer’s atomic volume graph (see chapter 4). The relationship Mendeleev had identified between volume and weight gave an argument for his system being a natural classification rather than an artificial one (1870a, 36). In a small note added to the end of the article, Mendeleev admitted that Meyer had made a similar observation independently, where he saw that the German chemist’s graph “(…) won in the clarity provided by the graphical illustration” (1870a, 49). Mendeleev’s article was published in Russian in 1870 in the Proceedings of the Congress of Russian Scientists.

Mendeleev’s next publication "On the Quantity of Oxygen in Saline Oxides and Atomicity of Elements” enforces the point that his aim of completeness called for accounting for many qualitative similarities between the elements (Mendeleev 1870b, 50-58). In this article, Mendeleev sought to give more evidence in favour of the naturalness of his system and to demonstrate periodicity more clearly by showing how degrees of oxidisation depended on atomic weights. Mendeleev had already noted the relationship between atomic weight and oxidisation in his earlier article on atomic volumes, but this article gave a more thorough treatment on the subject.

Mendeleev stated how his previous publication on atomic volumes (Mendeleev 1870a) succeeded in showing that the system corresponded with the physical properties of the elements, and how this suggested that his system was a natural classification. Mendeleev proceeded to give further evidence for the naturalness of his groupings by considering oxides (1870b, 51, 53). Elements could form oxides of many kinds, but only the most stable oxides could show gradualness in the properties of elements. Such stable examples were given by higher saline oxides, or oxides that provided salts (1870b, 51). Mendeleev organised them to the following schematic row:

R^2O	R^2O^2	R^2O^3	R^2O^4	R^2O^5	R^2O^6	R^2O^7
	RO		RO^2		RO^3	

From the periodic system, the first column corresponds to oxide of the form R^2O, the second column to RO (R^2O^2 divides to a pair of RO's), and so forth. For example, the elements on the second row of figure 5.3 provide the following higher saline oxides:

Na^2O	Mg^2O^2	Al^2O^3	Si^2O^4	P^2O^5	S^2O^6	Cl^2O^7
	MgO		SiO^2		SO^3	

When transitioning from left to right, the oxides formed by the elements changed from more basic to more acidic. Mendeleev brought attention to how this gradual transitioning from basicity to acidity complemented the natural groupings of elements (1870b, 54). Apart from the gradualness of the oxidisation, Mendeleev also noted that there was converse gradualness with hydrides: "[g]reater the atomic weight (…) the more oxygen and the less hydrogen it can hold in its compounds" (1870b, 57, see bottom row on figure 5.5). On these grounds, Mendeleev concluded that "[c]hemical properties of elements are the essence of the periodic function of their atomic weight" (1870b, 58).

As the gradual transitioning in higher saline oxides effectively drew attention to the naturalness of the arrangement, Mendeleev incorporated the schematic rows of oxides and hydrides in his new version of the system, which was published in his next article (see

the bottom rows figure 5.5.). The inclusion of the row of oxides shows how Mendeleev's consideration of chemical properties (such as consistency in forming saline oxides) in the course of developing his system also rendered completeness a design feature of his systems. Mendeleev included the two schematic rows in the majority of systems published in the course of his lifetime. For example, when we look in the system included in his textbook *Principles of Chemistry,* we can notice the two schematic rows on the top and formulae of compounds included under each of the elements, which shows that Mendeleev iterated with the system to make it more complete (see figure 5.9 at the end of this chapter).

This next article (which concerned the placing of cerium) marks a difference in Mendeleev's approach in developing his system. So far, we have seen how the gradualness of hydrides, chlorides, and oxides revealed the naturalness of his system (figure 5.2). We also saw how the relationship between specific volumes and atomic weights suggested that the system was a natural rather than artificial one. This suggests that the ability of the elements to form compounds with hydrogen, chlorine, and oxygen, together with their volumes, was taken into account in Mendeleev's broader goal towards a natural system of elements. More specifically, the above-mentioned qualitative properties guided Mendeleev's justification of his organisation of the elements.

However, in the article concerning the placing of cerium, we get the first example of how Mendeleev saw that the *data* needed to be changed to fit his organisation of the system of elements. More specifically, before introducing his new system, Mendeleev noted that periodicity called for changing some of the atomic weights of elements (Mendeleev 1871a, 59). Especially the weights of indium, uranium, and cerium appeared troublesome because the current weights forced one to place them in positions that did not fit their properties nor their oxidisation (Mendeleev 1871a, 59-60). This suggests that Mendeleev's goal of showing how various qualitative properties complemented his organisation had reached the point where he was confident that his system was natural rather than artificial. In fact, he was so confident in its naturalness that it warranted changing data that did not fit his organisation.

Mendeleev first considered changes to indium.[37] He noted that Bunsen's recent studies suggested that the heat capacity of indium was 0,057, which implied that its atomic weight had to be changed. Indium's atomic weight was taken as 75,6, but Bunsen's results showed that it ought to be changed to 1/3 75,6=113,4. The new weight would allow placing indium between cadmium (Cd) and tin (Sn) (see figure 5.4). This was compatible with data concerning its oxidisation.

In figure 5.4, the first row depicts atomic weights, the middle row specific weights, and the bottom row the steps in oxidisation that Mendeleev discussed in his previous article. It is visible that with the new weight of 113, the higher oxide of indium supports the gradualness of the arrangement. Further evidence for the suitability of the organisation was given by Mendeleev's replication of Bunsen's result, where Mendeleev's experiment suggested that the heat capacity of indium was 0,055 (1871a, 60).

Mendeleev then briefly discussed changes to uranium, where he suggested that its weights should be doubled to 240 (see also Scerri 2007, 128-131 for uranium). However, the main focus of the article was the corrections to cerium's weight and forms of its oxides. Mendeleev noted that cerium's widely accepted atomic weight of 92 did not find a place in the system. Mendeleev suggested changing the form of its basic oxide from CeO to

	Pl	Ag	Cd	In	Sn	Sb	Te	J
Атомный вес	106	108	112	113	118	122	‹125?›	127
Уд. вес	11,7	10,5	8,6	7,3	7,2	6,7	6,2	4,9
Высшая степень окисления, дающая соли	—	Ag^2O	CdO	In^2O^3	SnO^2	Sb^2O^5	TeO^3	J^2O^7.

Figure. 5.4 Table to illustrate the suitability of changes to indium's weight (1871a, 60)

[37] Mendeleev had written this article in late 1870, when he was already aware of Meyer's treatment of indium.

Ce^2O^3, which would change cerium's atomic weight to 3/2 92=138, and render the formula for higher oxide from Ce^3O^4 to Ce^2O^4. These changes would justify placing cerium after caesium (Cs=133) and barium (Ba=137) (Mendeleev 1871a, 62).

To demonstrate the suitability of these changes, Mendeleev enclosed a new version of the system, which included 64 elements if La, Di, Er are excluded from the calculation (see figure 5.5). On the bottom rows, we can see that Mendeleev had included the schematic rows of oxides and hydrides, where especially the former grounded Mendeleev's suggestion on the changes to the formula of cerium oxide.

While a more thorough explanation of the system would be given in Mendeleev's next article, here Mendeleev noted that it conveyed qualitative similarities both on vertical and horizontal lines (1871a, 65). For example, the elements on the evenly numbered horizontal rows had more metallic and basic character. The elements on the odd horizontal row were more acidic and had non-metallic properties especially on the higher rows. On the vertical columns, one could observe a gradual transitioning towards greater basicity (1871a, 65). We can observe this gradualness especially when we compare potassium, rubidium, and caesium (K, Rb, to Cs) and calcium, strontium, and barium (Ca, Sr, and Ba).

As an additional comment on the system, Mendeleev noted having excluded yttrium and erbium because their atomic weights and properties were not firmly established yet. This comment, together with the fact that Mendeleev assigned question marks to elements that would have their atomic weights corrected, suggests that he was drawing attention to the uncertainty of data in a manner akin to Meyer. Mendeleev mentioned having used whole numbers to express atomic weights because elements expressed with higher decimal points were uncertain (1871a, 65). Both attention to the quality of data and preference for rounded numbers is plausibly a response to Meyer's choice to use more precise atomic weights.

[453—454]

СИСТЕМА ЭЛЕМЕНТОВ

	Группа I	Группа II	Группа III	Группа IV	Группа V	Группа VI	Группа VII	Группа VIII (переход к группе I)
Типический ряд	H = 1 Li = 7	Be = 9,4	B = 11	C = 12	N = 14	O = 16	F = 19	
Ряд 1	Na = 23	Mg = 24	Al = 27,3	Si = 28	P = 31	S = 32	Cl = 35,5	
» 2	K = 39	Ca = 40	— = 44	Ti = 50?	V = 51	Cr = 52	Mn = 55	Fe = 56; Co = 59; Ni = 59; Cu = 63
» 3	(Cu = 63)	Zn = 65	— = 68	— = 72	As = 75	Se = 78	Br = 80	
» 4	Rb = 85	Sr = 87	(Yt? = 92?)	Zr = 90	Nb = 94	Mo = 96	— = 98	Ru = 104; Rh = 104; Pl = 106; Ag = 108
» 5	(Ag = 108)	Cd = 112	In = 113	Sn = 118	Sb = 122	Te = 128?	J = 127	
» 6	Cs = 133	Ba = 137	(Di = 143?)	Ce = 138	—	—	—	(La? Di? Er?) ————
» 7	—	—	—	—	—	—	—	
» 8	—	—	—	—	Ta = 182	W = 184	—	Os = 199?; Ir = 198?; Pt = 197?; Au = 197
» 9	(Au = 197)	Hg = 200	Tl = 204	Pb = 207	Bi = 208	—	—	
» 10	—	—	—	Th = 231	—	Ur = 240	—	—
Высшие формы окислов, дающих соли	R^2O	R^2O^2 или RO	R^2O^3	R^2O^4 или RO^2	R^2O^5	R^2O^6 или RO^3	R^2O^7	R^2O^8 или RO^4
Высшие формы соединений с водородом	—	—	RH^3?	RH^4	RH^3	RH^2	RH	—

Figure 5.5. Mendeleev's updated periodic system (Mendeleev 1871a, 66-67).

In conclusion to his article, Mendeleev argued that to justify his suggested changes to cerium's weight and forms of its oxides, he had experimented on cerium's heat capacity. His atomic weight correction anticipated the experimental results, but the findings were only preliminary, and a determination of greater exactness required more sensitive apparatus and purer samples of cerium (1871a, 65). Mendeleev left the reader with three concluding remarks: he noted that there were two elements missing on spots in groups III and IV; elements in group VIII displayed many similarities both individually and in their compounds; and, finally, natural groupings were complemented by the atomic weight ordering in the system.[38]

To sum up this section, I have argued that Mendeleev emphasised the importance of completeness in developing the early versions of the system. At first, the valuing of completeness was indicated by the high number of elements included in the first system. However, his subsequent publications suggest that completeness did not just involve including many elements, but also a thorough discussion of their various properties. This marks a striking contrast with Newlands' and Meyer's treatments. What forms the most pronounced difference with the systems of Newlands and Meyer, however, is that completeness was also a design feature of Mendeleev's systems. As we saw, he included the two schematic rows into the arrangement. In particular I have suggested that Mendeleev valued completeness in the development of these early versions because it allowed him to show that he had a *natural* system of elements. The emphasis on rigour allowed him to show that he had a natural *system* at hand.

As noted earlier, the new version of the periodic system (see figure 5.5) was the first to incorporate the two schematic rows of oxides and hydrides. Apart from providing evidence of the naturalness of the system, the row of oxides also guided Mendeleev's predictions of the properties of unknown elements. The predictions mark a difference between Mendeleev's early papers on the periodic system, and the final two articles: "Natural System of Elements," which has not been translated to English, and Mendeleev's seminal paper "Periodic Law of Chemical Elements," which included more thorough predictions. In

[38] This article was later translated to French and published eventually in *Mélanges physique et chimiques* in 1871 (Kedrov 1958a, 67).

what follows, I will discuss both in turn, while I will provide a more detailed treatment of Mendeleev's predictions to chapter 7 of this thesis.

5.3 From Naturalness to Lawfulness: Mendeleev's Seminal Articles on Periodicity

The chief aim of Mendeleev's next article was to highlight the naturalness of his system, which he did predominantly by emphasising the gradualness in how elements formed higher saline oxides (1871b, 69-101). Both of the seminal articles of 1871 synthesised Mendeleev's earlier discussion on how chemical and physical properties were distributed on the system. This again reinforces the impression that for Mendeleev, completeness did not just concern the quantity of elements, but also the consideration of various qualitative properties and similarities of the elements.

As with his first earlier article that elaborated on the "attempt" of a system, Mendeleev started with a brief evaluation of other systems on the market. He criticised those based on atomicity or valency, as he saw that they could be artificial systems at most, as they focussed on one property instead of many (1871b, 69). We can take this point as a subtle way of highlighting that a system had to take into account many properties of the elements. Although Mendeleev did not mention Meyer in this context, this claim can be taken as a comment on the artificial nature of Meyer's earlier systematisation of 1864, as it was predominantly guided by valency.

In contrast with the systems based on valency, Mendeleev advertised that his system was more rigorous system (*strongaya sistema*) than the other attempts (1871b, 69). However, despite having a more rigorous system at hand, Mendeleev revealed that there were two issues with his picture. Firstly, the accepted atomic weights of cerite metals (especially uranium and cerium) forced them to unsuitable spots in the system, which seemed to threaten the generality required for a natural systematisation (1871b, 69). The second issue concerned the layout of the original version presented in 1869. The original version had initially appeared to be a "convenient representation of all relations," but it placed together alkalines and haloids which were quite different. Both issues could be eliminated by offering an updated system of elements, which he had already presented in his previous article (1871b, 70, see also figure 5.5).

Before elaborating the features of the updated system, Mendeleev called for correcting the atomic weights of uranium, thorium, cerite metals halides, which he then proceeded to give. Like in the article discussed in the previous section, the row of oxides guided some of the suggested amendments, where Mendeleev saw that especially the changes to uranium's atomic weight and oxide provided new evidence of the schematic row of oxides being consistent with the chemical characteristics of oxides (1871b, 70-71). After demonstrating the relationship between the row of oxides in greater detail, Mendeleev concluded that the acceptability of the changes would only be confirmed with more substantial experimental results on uranium's heat capacity, isomorphism of its compounds, and its other properties (1871b, 71). Achieving such experimental back-up was challenging, though, because the samples of uranium were not pure enough.

Mendeleev then suggested corrections to the weights of thorium, cerite metals and halides with the help of the row of oxides (1871b, 72). Although often chemists assumed that the oxides of cerite metals had the form RO, the assumption made it difficult to place them in the system without disturbing the orderliness of the arrangement (1871b, 72). For this reason, Mendeleev proceeded to change the assumed forms of oxides. In this context, Mendeleev recapped his previous discussion on cerium, and argued that indium's form of oxide should be In^2O^3 and its atomic weight 113. The case of indium made Mendeleev.

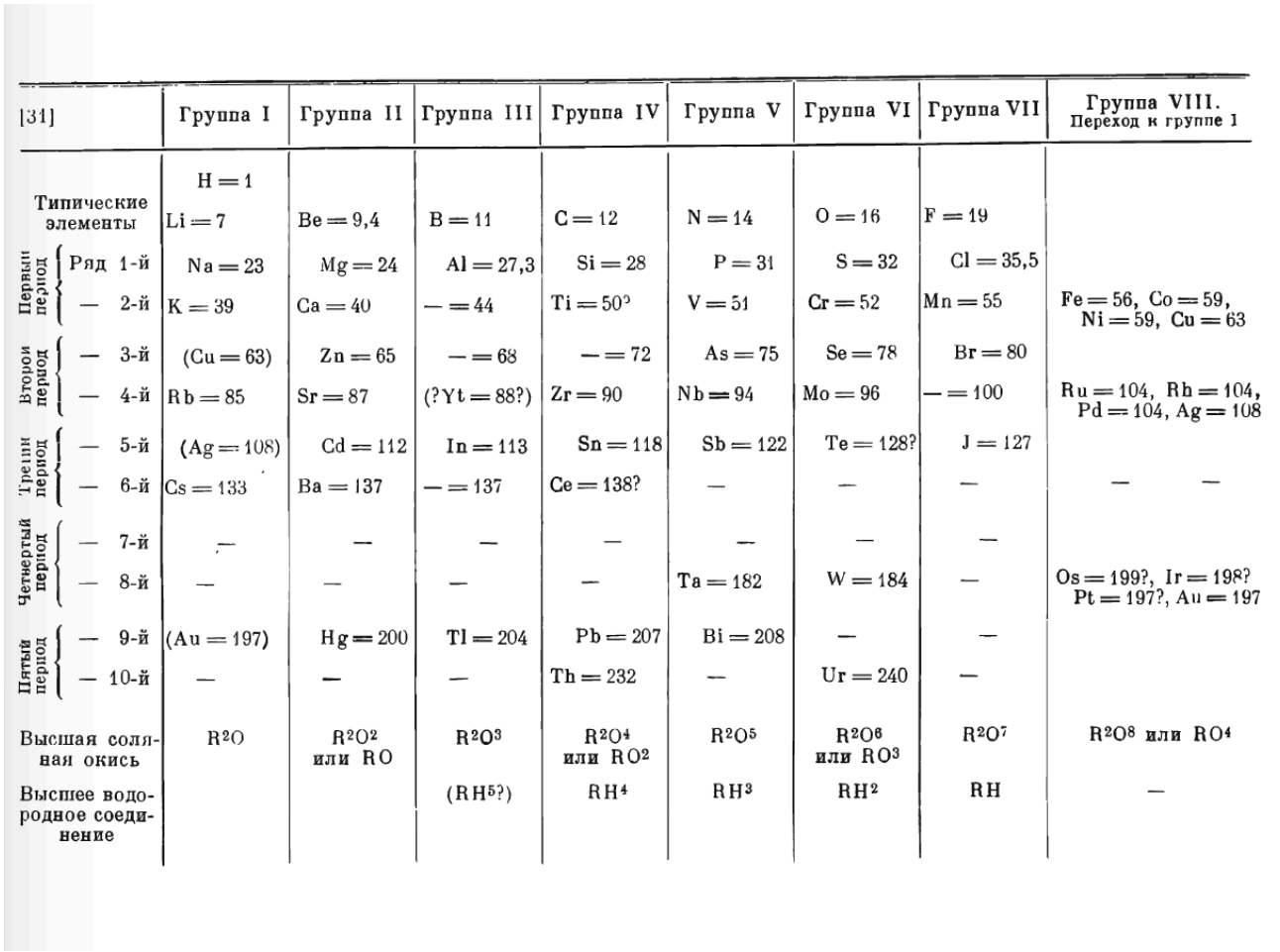
> even more certain of the rightness of the beginnings of periodicity. I dare say that the law of periodicity (i.e., the periodic dependency of the change of properties of the elements and their atomic weights) provides us a new means to determine the magnitude of the atomic weights of elements, because already in two examples, namely with indium and cerium, we saw the justification of the assumptions drawn on the basis of the law of periodicity. (Mendeleev 1871b, 74).

As Gordin notes, this was the first time that Mendeleev used “law” to refer to periodicity (2004, 31).[39]

After having identified a new position for the problematic elements that disturbed the arrangement, Mendeleev put forward a new periodic system which he proposed calling the natural system of elements. The name was justified as there were no foreseeable obstacles in “applying the system to the study of the properties of the elements and their compounds as in this system they are provided clear and sometimes even surprising simplicity and succession” (1871b, 75). He noted that the updated system could be used as a springboard for further investigations on elements, especially the study of unknown or little-known relations between elements (1871b, 75).

The new system bore a striking similarity to the one introduced in the article on cerium (figure 5.5). Most of Mendeleev’s elaboration of the system concerned how several chemical similarities could be represented systematically once the elements were ordered according to increasing atomic weight. In this system, similarities were conveyed on both vertical and horizontal lines, and in small and great periods. A smaller period was expressed on the row of “typical elements” (e.g. from lithium/Li to fluorine/F), where one could observe how “every seventh element is repeated” in the sense that they shared chemical properties (1871b, 75). On the second, fourth and eight rows, we encounter Mendeleev’s “greater periods” as the rows transition to a cluster of elements in Group VIII. Thus, for example, in the period starting with sodium (Na) there is no eighth after chlorine (Cl), but in the period beginning with potassium (K), we can find that it is followed by group VIII (1871b, 77). The greater period included roughly seventeen members.

[39] However, Mendeleev’s referred to his findings as a ”law” already in his earliest elaboration of the system, although he did not use the terminology of periodicity there yet (1869b, 21).



[31]		Группа I	Группа II	Группа III	Группа IV	Группа V	Группа VI	Группа VII	Группа VIII. Переход к группе I
		H = 1							
Типические элементы		Li = 7	Be = 9,4	B = 11	C = 12	N = 14	O = 16	F = 19	
Первый период	Ряд 1-й	Na = 23	Mg = 24	Al = 27,3	Si = 28	P = 31	S = 32	Cl = 35,5	
	— 2-й	K = 39	Ca = 40	— = 44	Ti = 50?	V = 51	Cr = 52	Mn = 55	Fe = 56, Co = 59, Ni = 59, Cu = 63
Второй период	— 3-й	(Cu = 63)	Zn = 65	— = 68	— = 72	As = 75	Se = 78	Br = 80	
	— 4-й	Rb = 85	Sr = 87	(?Yt = 88?)	Zr = 90	Nb = 94	Mo = 96	— = 100	Ru = 104, Rh = 104, Pd = 104, Ag = 108
Третий период	— 5-й	(Ag = 108)	Cd = 112	In = 113	Sn = 118	Sb = 122	Te = 128?	J = 127	
	— 6-й	Cs = 133	Ba = 137	— = 137	Ce = 138?	—	—	—	— —
Четвертый период	— 7-й	—	—	—	—	—	—	—	
	— 8-й	—	—	—	—	Ta = 182	W = 184	—	Os = 199?, Ir = 198? Pt = 197?, Au = 197
Пятый период	— 9-й	(Au = 197)	Hg = 200	Tl = 204	Pb = 207	Bi = 208	—	—	
	— 10-й	—	—	—	Th = 232	—	Ur = 240	—	
Высшая соляная окись		R^2O	R^2O^2 или RO	R^2O^3	R^2O^4 или RO^2	R^2O^5	R^2O^6 или RO^3	R^2O^7	R^2O^8 или RO^4
Высшее водородное соединение				(RH^5?)	RH^4	RH^3	RH^2	RH	—

Figure 5.6 Mendeleev's first system of 1871 (1871d, 76).

Mendeleev also drew attention to the distinct character of the periods on even and odd rows. Elements on the even rows tended to be more basic, but there were also gradual differences on the vertical lines, as the elements lower down on the groups appeared more basic (1871b, 78). For example, caesium, a member of the even row 6, seemed more basic than rubidium and potassium above it, and in group II, barium, a member of the even row 6, appeared more basic than strontium and calcium (1871b, 78). A similar trend is repeated within group IV when moving from thorium to cerium, and then from zirconium to titanium. Furthermore, Mendeleev noted that similar gradualness was visible when transitioning from uranium (Ur) to tungsten (W), molybdenum (Mo) to chromium (Cr). Another significant similarity between elements on even rows was that none of them provided organometallic compounds nor hydrides (1871b, 78). This formed a contrast with the odd rows, where many of them provide organometallic compounds (1871b, 78-79).

After having elaborated on the distinct character of the even and odd rows, Mendeleev considered the groups on the vertical columns in greater detail. He argued that elements in each group formed higher oxides, hydrides, and chlorides in a principled manner that could be represented with a schematic compound form. Thus, for example in group I, all elements – both on odd and even rows – provided higher oxides of the form R^2O, chlorides of the form RCl, and were unable to provide hydrides (1871b, 81). Group II elements provided higher oxides of the form RO, chlorides of the form RCl^2, where they did not combine with hydrogen. The gradualness becomes clear when we note that group III elements give oxides of the form R^2O^3 and chlorides of the form RCl^3.[40]

Mendeleev then explained how elements in group IV gave oxides of the form RO^2, chlorides of the form RCl^4. Hydrides had the form RH^4, although only carbon and silicon provided hydrides (1871b, 84). According to Mendeleev, it was "very important to note" that amongst all of the groups, this group showed greatest similarity in their "atomic composition." This was because all of the abovementioned compounds were "four-atomic," or had the valency of four (1871b, 84). The gradualness in transitioning from one group to another becomes clear when we inspect next collection of elements, the group V: there, Mendeleev highlighted that members provided oxides of the form R^2O^5, hydrides of the form RH^3 and chlorides of the form RCl^5. In the lower steps of oxidisation, the elements in this group often took the form RX^3, which marked a similarity with the formula for their hydrides (1871b, 87).

Mendeleev's discussion of the remaining groups was brief: he only noted that in group VI, members provided hydrides of the form RH^2 and quite acidic higher oxides of the form RO^3. All the members in group VI provided various compounds of the form RX^2 or RX^4 (1871b, 87). Elements in group VII provided hydrides of the form RH, and oxides of various forms, where the highest step of oxidisation is R^2O^7 (1871b, 87). With the final group, Mendeleev only claimed that its members provided unstable compounds with hydrogen and various non-volatile, dissolvable compounds (1871b, 87). Apart from the

[40] In this context, Mendeleev included an intermission to consider the chlorides as they seem to threaten the gradualness of the transition (Mendeleev 1871b, 82-84).

different chemical properties discussed here, Mendeleev also noted that periodicity is expressed by specific volumes and other physical properties both of individual elements and their compounds (1871b, 88).

This discussion of oxides gives a sense of how Mendeleev aimed for completeness in discussing qualitative similarities. This forms a contrast especially with the approach of Newlands, who only noted that there were similarities to be found from his system. What also makes Mendeleev's approach different from that of Newlands is his demonstration of how new elements would be incorporated to the arrangement. The fact that Newlands' arrangement was criticised for its incomplete appearance and for not making it clear where to put undiscovered elements suggests that completeness called for accounting for undiscovered elements, too (Stokes 1887, 195).

While both Meyer and Mendeleev (and Newlands's paper of August 1864) made it explicit where to place the elements that would be discovered in a later occasion, only Mendeleev used the periodic system to offer detailed predictions of the properties of unknown elements. In this article, Mendeleev was the first to use his system to predict properties of unknown elements. After having offered a description of the properties of missing elements (eka-aluminium, eka-gallium, and eka-boron), the seminal paper of 1871, "Periodic Law of the Chemical Elements" made these predictions more thorough. The title of the article shows that it was not enough for Mendeleev to highlight the naturalness of his system, but that he had identified a natural law.

The seminal paper of 1871 is the most substantial of Mendeleev's articles on the periodic system. It is organized in six sections, where the first articulates the essence of periodic law, and the rest concern its application.[41] Mendeleev started by calling for a clearer distinction between "simple substances" and "elements," where he argued that "simple substances" were material and had various properties, and "element" corresponded to the idea of an atom (1871c, 102). The material particles of a specific element determined the

[41] The applications of the "periodic law" include systematising the elements, determining atomic weights and properties of undiscovered elements, correcting the magnitude of atomic weights, and completing knowledge of the forms of chemical compounds.

physical and chemical behaviour of simple substances and compounds (1871c, 102). This distinction allowed Mendeleev then to articulate that the goal of chemistry was to "extend our knowledge of the relations between the composition, the reactions, and the qualities of simple and compound substances on one hand, and the intrinsic qualities of the elements which are contained in them, on the other" (1871c, 102). Gaining such knowledge of the simple substances and the qualities of elements allowed then to deduce "all the properties of all its compounds" including the unknown ones (1871c, 102).

For Mendeleev, from all of the properties of elements (as opposed to simple substances) only two lent themselves for precise measurement: atomic weights of elements and capability to form compounds (i.e. the valency of elements). Mendeleev saw that physical properties were not "yet in that level of precision and generality that would allow for creating a complete scientific system" (1871c, 103).[42] Knowledge on atomic weight and valency was more developed, but the available data on physical properties was well-developed enough so that one could notice a dependency between them and atomic weights (and molecular weights of compounds) (1871c, 103).

Mendeleev then proceeded to chemical properties. He noted that they were only considered as qualitative properties for the time being, because they could not yet be measured (1871c, 103-104).[43] In Mendeleev's own words: "Absence of [exact] measurements of these properties renders them unsuitable for generalizing chemical findings; considerations that are only built on these properties, will always suffer from unsteadiness" (1871c, 104). Despite this defect, he stressed how qualitative properties should not be left "out of sight" as "many sides of chemical findings can be generalized with their help" which again gives us an idea of his valuing of completeness in establishing the system (1871c, 104).

[42] As to specific physical properties, Mendeleev listed cohesion, heat capacity, coefficient of refrangibility, and spectral phenomena in this context.
[43] In this context, he noted that they included at least basicity, acidity, the capability to combine (or not combine) with hydrogen and chlorine, and the capability to provide salts.

Apart from calling chemists to keep in sight qualitative properties, Mendeleev also suggested considering both the properties that were unique to them and they shared with others:

> If the wish to generalize the properties of elements, to subject them to rigorous investigations that would allow for practical conclusions and chemical predictions, it is necessary to pay attention both to shared properties that belong to the particular group of elements … and to the individual properties (1871c, 104).

This demonstrates that completeness in accounting for different properties was not just a matter of ensuring that he had a natural systematisation of the elements at hand. Instead, aiming for completeness helped at rendering the system useful for more practical endeavours and making predictions.

Although Mendeleev concluded this discussion with the foreseeable point that only atomic weights offered a firm foundation for systematisation, crucially, he also argued that the best way of pursuing a system was to see how the other properties of elements depended on the atomic weights (1871c, 105). In particular, Mendeleev wanted to explore the dependency between atomic weights and their valency. However, the studies on valency expressed uncertain results as even the valences of well-known elements like sodium (Na) and chlorine (Cl) was a matter of controversy, as some investigators regarded their valency as a stable property of the elements and others as something that changed according to the properties of elements (1871c, 105). From the point of view of completeness, Mendeleev's diagnosis of the uncertainty of the studies on valency is telling; he worried that valency had been investigated in isolation from other properties of elements (1871c, 105). Mendeleev's recommended remedy was to study the dependency between *many* properties of elements and their atomic weights, not just one. This emphasis on focussing on "many" over the "few" demonstrates how Mendeleev sought to foreshadow the greater completeness of his system.

Mendeleev then elaborated how the previous attempts of mapping the relationship between atomic weight and other properties had drawn attention to the relationship

between the atomic weights of elements belonging to a natural group.[44] However, Mendeleev saw three problems in particular in their works (1871c, 106). Firstly, they did not tie together all known natural groups into one whole, but left the relationships between the individual members of groups as unexpected and inexplicable phenomena (1871c, 106). Secondly, investigators had given attention mostly to similar elements with close atomic weights. This problem was related to the third issue; the investigations to the "simple and exact" relations in the atomic weights of dissimilar elements were scarce. Mendeleev stressed that the investigation of relations between dissimilar elements enabled identifying the "right" (*pravil'noye*) relationships between the changes in the atomic weights and the other properties (1871c, 106-107). Again, this discussion illustrates Mendeleev's call for completeness in considering both for many elements and similarities between all kinds of elements – not just those that had similar atomic weights.

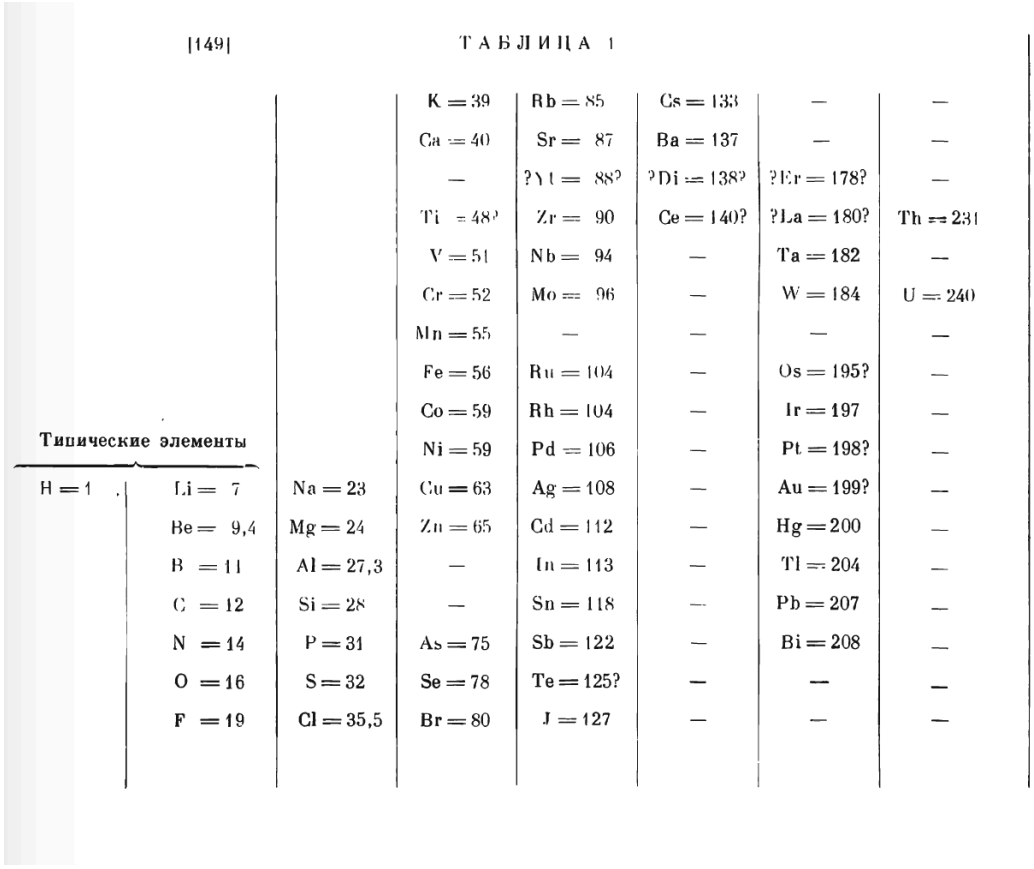
This preface alone gives us five different indicators of Mendeleev's valuing of completeness. Firstly, Mendeleev stated that the goal of chemistry was to extend knowledge of elements, and simple- and complex substances. The second (and an especially telling) indicator of completeness can be found from Mendeleev's assessment of the suitability of physical properties as organising principles. As we saw, Mendeleev stated that physical properties did not allow for creating a *complete* scientific system. The third clear indicator of Mendeleev's valuing of completeness is that he called to keep in sight both chemical and physical properties, and both the individual and shared properties of the elements when systematising them. The final indicator of completeness in this context can be found from Mendeleev's discussion of valency as an organising principle for a system. As we saw, he noted that the study of valency had given uncertain results primarily because investigators had not considered many properties of the elements, but focussed on valency in isolation from other properties.

After highlighting the analogical relations between elements that he had already discussed in his previous articles, Mendeleev considered the gradual transitioning that his systems expressed (1871c, 109-110). In this discussion, he introduced the two versions of his

[44] Here, Mendeleev referred to the studies of J.H. Gladstone, J.P. Cooke, M.Pettenkofer, P.Kremer, J.B. Dumas, E.Lenssen, W.Odling in particular.

periodic system, where both systems included 63 elements. Interestingly, both tables show that Mendeleev highlighted the differences in the quality of data. For example, Mendeleev noted how he had expressed the atomic weights in round numbers as he saw little certainty in the accuracy of the atomic weights expressed with decimal numbers (1871c, 115). This is most likely a response to Meyer's use of more precise atomic weights.

Furthermore, what also illustrates Mendeleev's signalling of the quality of data is that again, in some cases, he included question marks both before the symbols of the elements and after their atomic weights. If the question marks were placed before the symbols, they indicated that they might be relocated on the table once their properties became better known. If the question mark stood after the atomic weights, it indicated doubt in the atomic weight determination. Mendeleev noted having changed the weight of some of the elements so as they accorded with the periodic law. For example, tellurium is changed from 128 to 125.



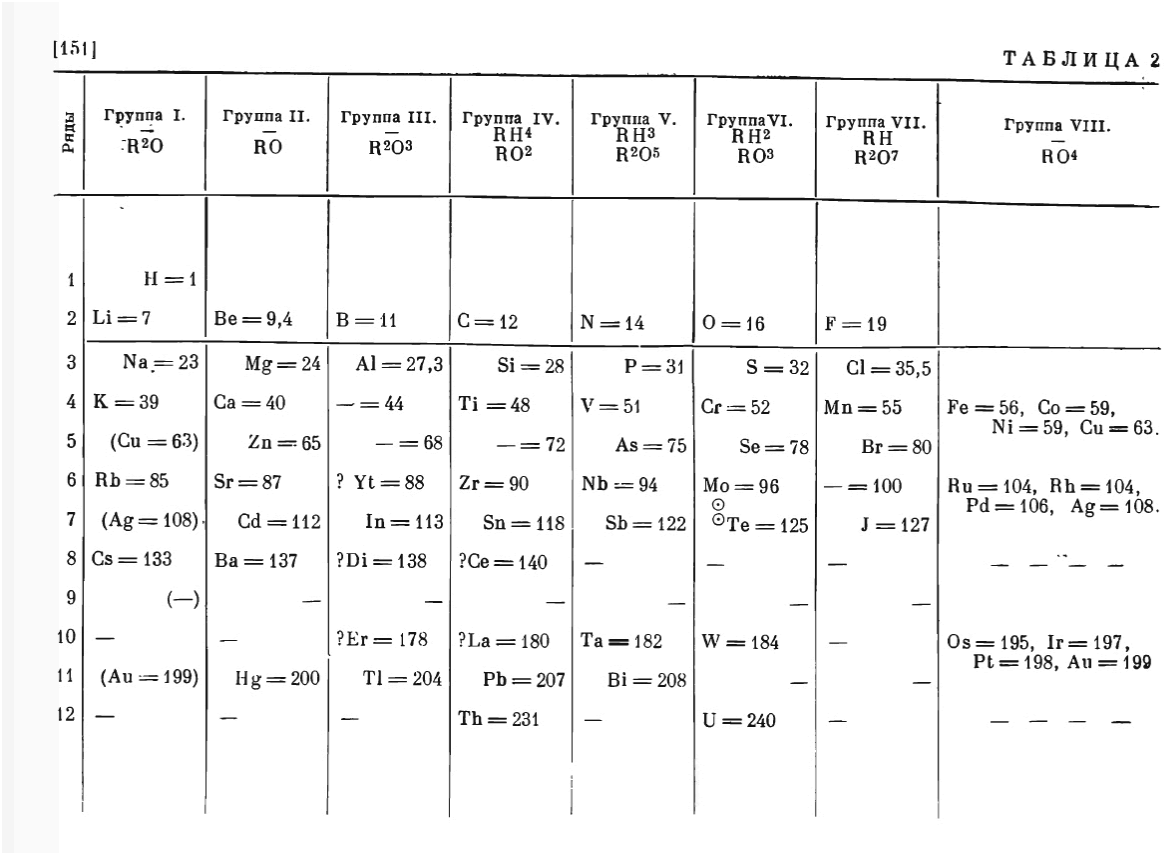
|149|

ТАБЛИЦА 1

			K = 39	Rb = 85	Cs = 133	—	—
			Ca = 40	Sr = 87	Ba = 137	—	—
			—	?Yt = 88?	?Di = 138?	?Er = 178?	—
			Ti = 48?	Zr = 90	Ce = 140?	?La = 180?	Th = 231
			V = 51	Nb = 94	—	Ta = 182	—
			Cr = 52	Mo = 96	—	W = 184	U = 240
			Mn = 55	—	—	—	—
			Fe = 56	Ru = 104	—	Os = 195?	—
			Co = 59	Rh = 104	—	Ir = 197	—
Типические элементы			Ni = 59	Pd = 106	—	Pt = 198?	—
H = 1	Li = 7	Na = 23	Cu = 63	Ag = 108	—	Au = 199?	—
	Be = 9,4	Mg = 24	Zn = 65	Cd = 112	—	Hg = 200	—
	B = 11	Al = 27,3	—	In = 113	—	Tl = 204	—
	C = 12	Si = 28	—	Sn = 118	—	Pb = 207	—
	N = 14	P = 31	As = 75	Sb = 122	—	Bi = 208	—
	O = 16	S = 32	Se = 78	Te = 125?	—	—	—
	F = 19	Cl = 35,5	Br = 80	J = 127	—	—	—

Figure 5.7 Mendeleev's second system of 1871 (1871c, 114).

The second table (figure 5.8) expressed both the small and great periods introduced in the earlier publication. Mendeleev recapped his previous observation of how the even series had more metallic character, where the members formed no volatile or organometallic compounds. The odd series had more acidic properties (1871c, 117).[45] In contrast with his previous introduction of this table (figure 5.6), however, Mendeleev explained why copper (Cu), silver (Ag), and gold (Au) appeared on the system twice both in group VIII and group I. Mendeleev explained having located them in group VIII because of similarities between cuprous and aurous oxide, and CuCl, AgCl and AuCl. As the elements also



[151] ТАБЛИЦА 2

Ряды	Группа I. $\dot{R}^2O$	Группа II. $\dot{R}O$	Группа III. R^2O^3	Группа IV. RH^4 RO^2	Группа V. RH^3 R^2O^5	Группа VI. RH^2 RO^3	Группа VII. RH R^2O^7	Группа VIII. $\dot{R}O^4$
1	H = 1							
2	Li = 7	Be = 9,4	B = 11	C = 12	N = 14	O = 16	F = 19	
3	Na = 23	Mg = 24	Al = 27,3	Si = 28	P = 31	S = 32	Cl = 35,5	
4	K = 39	Ca = 40	— = 44	Ti = 48	V = 51	Cr = 52	Mn = 55	Fe = 56, Co = 59,
5	(Cu = 63)	Zn = 65	— = 68	— = 72	As = 75	Se = 78	Br = 80	Ni = 59, Cu = 63.
6	Rb = 85	Sr = 87	? Yt = 88	Zr = 90	Nb = 94	Mo = 96	— = 100	Ru = 104, Rh = 104,
7	(Ag = 108)	Cd = 112	In = 113	Sn = 118	Sb = 122	⊙Te = 125	J = 127	Pd = 106, Ag = 108.
8	Cs = 133	Ba = 137	?Di = 138	?Ce = 140	—	—	—	— — — —
9	(—)	—	—	—	—	—	—	
10	—	—	?Er = 178	?La = 180	Ta = 182	W = 184	—	Os = 195, Ir = 197,
11	(Au = 199)	Hg = 200	Tl = 204	Pb = 207	Bi = 208	—	—	Pt = 198, Au = 199
12	—	—	—	Th = 231	—	U = 240	—	— — — —

Figure 5.8 Mendeleev's third system of 1871 (1871c, 116)

[45] There was one objection to this generalisation: the second series show acidic properties and give organochemical and hydrogen compounds (i.e., they behaved like elements in the odd series). However, Mendeleev pointed out that unlike elements in even series, this second series is not connected to group VIII, and the weights of the elements in it differ from the following series by 16 rather than 24 or 28, which marks it as different from other even series (1871c, 117).

formed an analogy with compounds of elements in group I (e.g. H, Na, Cu, Ag, Au), he included them there, too (1871c, 119).

On the first of the two tables, Mendeleev noted how the great periods started with reactive alkalis and then ended with reactive haloids. “Electrochemical findings” suggested that the first members of the great periods had quite reactive chemical characteristics, or they reacted easily with other bodies without requiring high temperatures. Furthermore, they had quite close atomic weights (1871c, 120). When moving from the middle of the system to the ends of horizontal rows, Mendeleev noted that one could find qualitatively different elements (1871c, 121). In the middle of the system, after alkalis and alkali earths, one could encounter rarer elements that had similar properties. More specifically, they appeared non-volatile, refractory, not very reactive, could often be found together, and were only rarely found in large quantities (1871c, 121). Mendeleev concluded by pointing out that the data on the rare elements was far from complete. Had it not been for the works of Marignac on zirconium (Zr), niobium (Nb), and tantalum (Ta), then these elements would not have found a meaningful place in the system (1871c, 122).

Mendeleev concluded the discussion of the systems by stating that the significance of “a law of nature” was only established after it gave rise to new practical investigations, which would help to both establish the usefulness of the law and test its rightfulness (1871c, 123). For this reason, the rest of the paper concerned the applications of the law of periodicity. The applications included determination of the properties of little-known and unknown elements; improving the atomic weights, and supplementing information on the forms of chemical combination, where Mendeleev first discussed its “application to systematicity” (1871c, 124).

The phrasing concealed that Mendeleev had in fact first started by systematising the elements, which only subsequently gave rise to the finding that he titled the “law of periodicity.” This *post hoc* application of the periodic law, however, reveals that by the time of writing this article, Mendeleev no longer saw himself as being in the business of establishing just natural systems. Instead, the periodic law provided “simple quantitative guidance” for distributing the elements, and afforded more perfect or complete

(*sovershennaya*) systematisation that lacked in all arbitrariness of the other systematisations (1871c, 126). (Mendeleev's applications of the law to the determination of the properties of little-known and unknown elements will be the subject of chapter 7 of this thesis.)

5.4 Conclusion

In this chapter, I have brought attention to the many ways in which Mendeleev emphasised completeness when developing his systematisation of the elements. In particular, I have identified the following six indicators of completeness: (1) the large number of elements included in the system, (2) discussion on how at least seventeen properties found a suitable place in the system,[46] (3) emphasis on the importance of completeness (*polnost'*) for a systematisation, (4) suggesting places for undiscovered elements, (5) incorporating more information (e.g. the row of oxides) to the visual layout of the system, and (6) evaluating the previous systematisation attempts in terms of completeness. Despite Mendeleev's shift from aiming for natural system to the identification of law of nature, we saw that initially, completeness with respect to elements and their properties was required to show that he had achieved in making a natural rather than an artificial systematisation.

While the emphasis on completeness makes Mendeleev's system distinctive, it is important to note that it was not the only valued quality that Mendeleev evoked. We saw that he called for rigorousness and occasionally referred to the simplicity of periodicity. Furthermore, especially in the articles Mendeleev published after acquaintance with Meyer's graph of 1870, Mendeleev started emphasising carefulness in distinguishing between different kinds of data. However, his inclusion of many elements from the get-go, together with the breadth of his discussion of the spread of different qualitative properties suggest that completeness was the most dominant value guiding his systematisation.

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[46] The above chapter included Mendeleev's discussion of valency (or atomicity), steps of oxidisation (how some elements shared the same form of oxide, and the gradual transitioning across the system in how much oxygen the elements could combine with); basicity and acidity, metals and metalloids, compounds with fluorine, compounds with nitrogen, compounds with ammonia, hydrides, isomorphism, homologues, atomic volumes, electrochemical behaviour, organometallic compounds, volatility, refractability, reactivity, and how rare or typical the elements were.

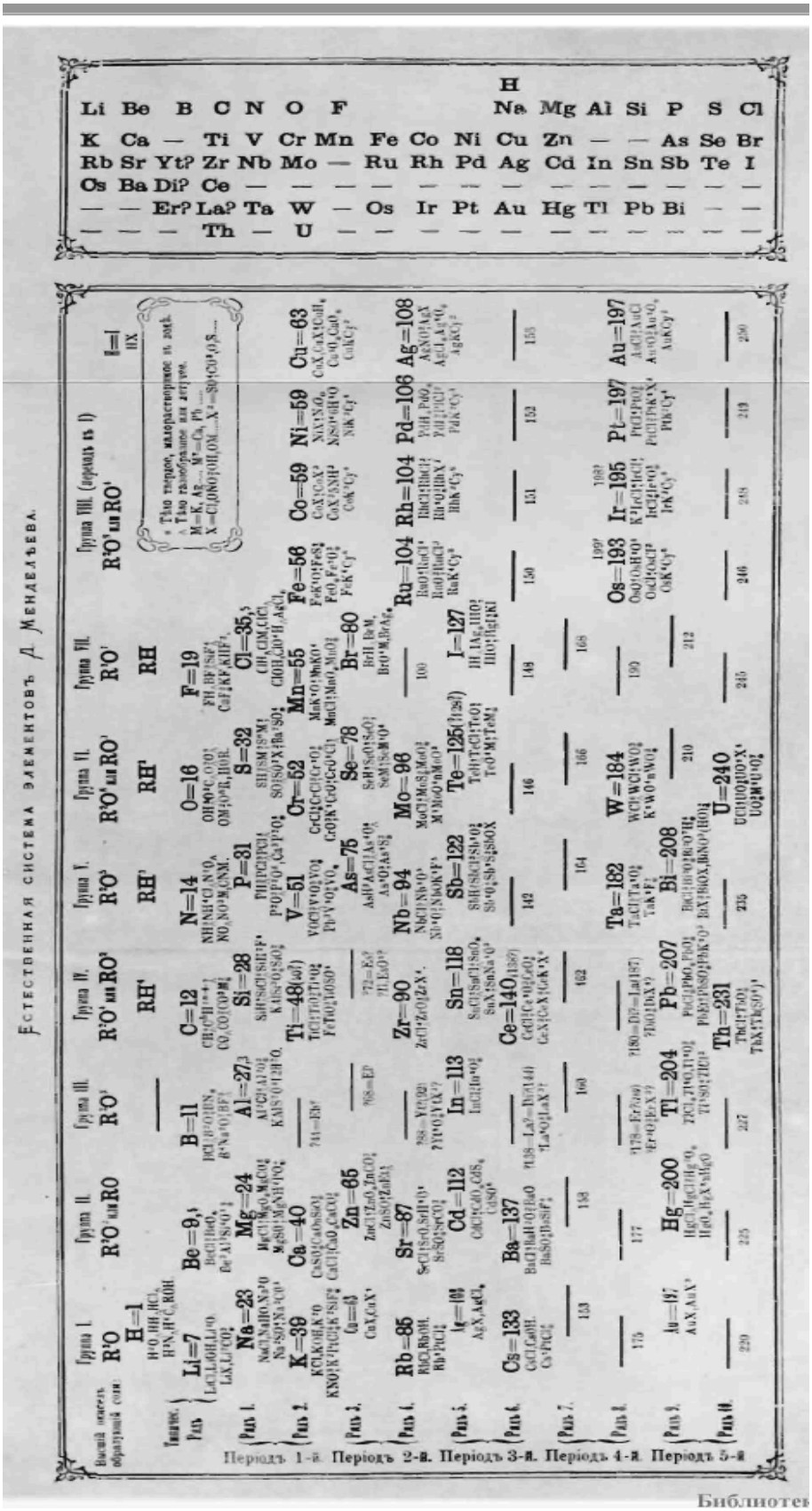


Figure 5.9 System included in the textbook *Principles of Chemistry* (Kedrov 1958a, 341).

Chapter 6. Abstract Analysis of Carefulness, Simplicity, and Completeness

6.1 Introduction

In chapters 3-5, I detailed how Newlands, Meyer, and Mendeleev valued simplicity, completeness, and carefulness in the course of developing their systematisations of the chemical elements. In this chapter, I provide a more abstract characterisation of each value and argue in favour of adopting my proposed analytic categories to articulate the distinctness of the systems of Newlands, Meyer, and Mendeleev.

First, I should reflect on why it may be beneficial to provide more thorough abstract definitions of values only after presenting the historical material. One argument in favour of such an approach can be found in G.C. Field's 1931 essay on the practice of defining in moral philosophy. Field (1887-1956) shunned the desirability of giving definitions at the start of investigations. Instead, Field argued that the ethicist should arrive at a definition at the end of their investigation after having been receptive to the moral experience of people and examining the assumptions that underlie morality (Field 1931, 90-91). After such initial examinations, the "task of the moral philosopher is to make clear and explicit the vague ideas that are held about the objects with which he deals. He has to discover as much as he can of what is implied by the ways in which the chief moral notions are or have been used" (Field 1931, 90).

Similarly, in investigating values in science, it serves well to first start with examining values in practice and produce a definition (or a characterisation) of values only at the end of the investigation. In this way, the end-goal of the investigation would be (at best) an illuminating definition that effectively reveals the underlying presuppositions of the concrete instances it seeks to clarify. By doing so, it can expose novel information about values in science. Gaining new information about the role of values would be less likely if we employed a rigid definition at the very start of the investigation and test if the specifics of the concrete case could be squeezed into its template. Such an approach could, at

best, tell us about the generality of some definition (for philosophers' generalisations about science, see Sauer and Scholl 2016).

For this reason, I provide a more thorough abstract characterisation of each value only after describing the particulars of the historical case. The division of labour between the chapters reflects Chang's articulation of the relation between history and philosophy of science (Chang 2012b). As Chang notes, traditionally, in HPS investigations authors have taken history to concern the particular, and philosophy the general, but this conception has led to issues which are broadly known as the problem of case studies: how much can we generalise from a handful of them? (Scholl and Sauer 2016, 1-8; 60-01).

Instead of opting for the traditional division of labour where history is viewed as the source of the particular cases and philosophy the source of the generalisations, Chang argues that we should take the relation between history and philosophy of science as that between concrete and abstract (Chang 2012b, 110). Such a way of conceiving their relationship is more helpful, because "[a]bstract ideas are needed for the understanding of any concrete episode, so we could not avoid them even if we only ever had one episode to deal with (…) Any concrete account requires abstract notions in the characterization of the relevant events, characters, circumstances and decisions" (Chang 2012b, 110). As the aim of this thesis is to offer an interpretation of values in the development of the periodic systems, it makes more sense to provide an abstract characterisation only after a detailed study of the concrete episode. This is because the converse approach would lead to a testing of a general category rather than an illumination of values in the concrete case.

As we saw, chapters 3-5 of this thesis brought attention to the concrete instances of valuing during the development of the periodic systems of elements, so now it would be appropriate to suggest a more abstract characterisation of values. However, as Schickore's hermeneutic model called for, our analytic categories (and abstract characterisations) should be especially useful at elucidating the concrete case in question. In Chang's terminology, our (philosophical) abstract characterisations should be cogent, where their cogency can be tested through further abstract considerations and arguments (Chang 2012b, 111, see pp.112-121). In Chang's own two examples, the cogency of the

abstract proposals can be tested by seeing how coherent and general they are. In this chapter, I propose an additional way to test the cogency of abstract characterisations (or analytic categories). I suggest we can test their aptness through comparing them with other closely-related abstract characterisations. Such comparisons would give us an idea how they fare at elucidating the historical episode at hand.

The structure of this chapter is as follows. In section 6.2, I argue in favour of my proposed understanding of carefulness, and show how the concrete instances of valuing found in Meyer's articles give rise to the definition. After doing so, I test the cogency of carefulness as an analytic category through comparing it with other closely related analytic categories that could be applied to account for Meyer's approach. In particular, I argue that sharpness, accuracy, and precision do not fare quite as well as carefulness in explicating Meyer's approach. In section 6.3, I then discuss "simple relation" of Newlands' systems. Contrary to carefulness and completeness, I demonstrate how it is challenging to provide a consistent abstract analysis of Newlands' "simple relation," as Newlands evoked different senses of simplicity. In section 6.4, I show how to update Chang's (2012a) definition of completeness. The updated definition will help to make sense of different kinds of completeness of the systems of Meyer, Newlands, and Mendeleev, and highlight how Mendeleev emphasised completeness more than the other two chemists.

6.2 Carefulness

In chapter 4, I argued that Meyer valued carefulness in developing his systematisation of the elements. In the beginning of the chapter, I advertised that I had adopted the following definition of carefulness:

> *Carefulness involves signalling differences in the quality of observations of the phenomena on the problem area, and prioritizing observations of higher quality over those that are lacking.*

It is crucial to note that this definition of carefulness is intended to explicate Meyer's selection of observations that he used as a basis of the periodic systems. This is not to deny that other features of Meyer's approach in chemistry could not be described as

careful in the more colloquial sense. However, if we were to apply the category of carefulness to analyse Meyer's approach to theories, I suggest we should employ my proposed (more technical) understanding of carefulness rather than the more colloquial understanding of carefulness (which connotes timidity or caution). I suggest that my account of carefulness is more suitable to characterise Meyer's approach to theories, because (as I briefly noted in chapter 4) Meyer was more willing to defend theoretical assumptions than Mendeleev, where this discrepancy between their approaches problematises the analyses which characterise Meyer as the timid one and Mendeleev as the bold maker of predictions (Gordin 2012). If we were to apply the category of carefulness to Meyer's approach to theories more broadly, we need to be mindful of how the colloquial understanding of carefulness might mischaracterise Meyer's views.

Furthermore, although in its colloquial usage, the term 'carefulness' usually describes the character of an individual, I maintain that it can also be used to describe the works or products of the individual (or groups of individuals). This should not be unintuitive. For example, a political journalist who describes the government's policy as carefully drafted is likely to base their description not on the personal qualities of the team of politicians, consultants, or civil servants who created the policy, but on the properties of the documents detailing the policy. The description of the journalist might imply that the team who drafted the policy have been careful, but more often than not, such descriptions are based on some properties of the documents stating the policy rather than the journalist's fly-on-the-wall observations on *how* the team has drafted the policy. In a similar vein, we can also detect carefulness in the products of science.

Although the indicators of carefulness in scientists' products can hint of the carefulness of the individuals who developed the product, I suggest we should remain agnostic about the carefulness of the individuals, especially in cases where our sources do not explicitly refer to the personal traits of the individuals. This is not to say that there is no possibility that Meyer was a careful individual. Rather, it is far less of a stretch to say that Meyer's systems are careful in distinguishing between different kinds of observations than to maintain that he was a careful individual. Although it might strikes us as an ungrammatical way of using the term, such turn of phrase emphasises that we are not in the business of

identifying the virtues of the investigator, but values of systems. I maintain that in the light of the textual evidence, we should remain agnostic about Meyer's personal traits. This does not imply that we could not provide a substantial analysis of the traits of his works.[47]

The definition of carefulness I proposed above has two components. The first part of the definition focusses on discerning different kinds of observations on the phenomena of the problem area.[48] One obvious example of this from the context of systematising the elements are atomic weights. As I noted in chapter 2, errors in atomic weight determinations could be caused either through the experiment itself or from a problematic assumption of the formula for the combination of elements. Furthermore, this first part of the definition also involves signalling the high quality in observations. For example, in 1888, Meyer noted that atomic weights that were determined from many compounds of elements increased the accuracy of the results (Meyer 1888, 83).[49] The second part of the definition picks up the prioritisation of the good-quality observations over those that were lacking.

1.	2.	3.
Expressing atomic weights in an exact form.	Making uncertainties explicit.	Exclusion of elements that were not well-known.

Figure 6.1 The Indicators of carefulness

[47] Some historians have investigated the traits of individual investigators, where they would tend to use the terminology of sensibilities, mentalities, or mental tools, see Olesko (1995), Thomas (1983, 15-16), and Chartier (1988, 24-25).
[48] Although I have used the terminology of observations in the definition of carefulness, I should highlight that this is not intended as a statement on the possibility of a clear demarcation between observation and theory. If anything, errors in atomic weights pushes for the familiar point that observations are theory-laden (Hanson 1958) as assumptions on the formulae could influence what atomic weights were (see chapter 2; Chang 2012a, xvii).
[49] In this context, Meyer especially praised the atomic weight determinations of chlorine, titanium, chromium, rubidium, strontium, and barium.

In order to make explicit the link between the abstract definition of carefulness and the concrete features of Meyer's system, I will now explain how the definition relates the three indicators of carefulness I identified in chapter 4 (recapped on figure 6.1). From the three indicators of carefulness, (1) and (2) relate to the first part of the proposed definition. For example, Meyer's use of precise atomic weights in conjunction with less-precise ones signalled the quality of observations of some atomic weight determinations. The precision of the atomic weights highlighted the experimental (and conceptual) success of the chemists determining them, which gives us the first indicator of the valuing of carefulness in Meyer's approach. In contrast to the first indicator, the second indicator signalled when some observations were flawed or more uncertain than others. For example, Meyer expressed different kinds of data with different lines on his atomic volume graph. For the versions that he included in the textbook, he included large gaps when he saw that the evidence was not sufficient to warrant drawing any line at all. Together, the two indicators helped Meyer to differentiate between observations of different quality.

However, while Meyer included elements that were not equally well characterised to the same system, there were some elements with properties so uncertain that Meyer chose to exclude them entirely. For example, in 1870, Meyer omitted elements which did not have atomic weights determined or where their atomic weight determinations had been uncertain. The exclusion gives us the third indicator of carefulness of Meyer's systems, where it attaches to the latter part of the definition of carefulness given above (i.e., *prioritizing observations of higher quality over those that are lacking*). Exclusion of some elements shows that Meyer prioritised those elements that he saw as sufficiently well-characterised.

In short, the suggested definition of carefulness arises from the concrete instances of valuing that I brought forward in chapter 4. The main benefit of this abstract characterisation of carefulness is that it reveals the underlying theme of Meyer's iterations when he developed his systematisation. In other words, employing the more abstract notion of carefulness helps to highlight that Meyer's exclusion of some elements, his use of dotted and continuous lines, and his highlighting of uncertainties in the experimental

results were not isolated technical choices, but that they all were underpinned by Meyer's deeper considerations on the quality of observations. On these grounds, the abstract definition of carefulness put forward here satisfies Field's condition for definitions: it renders explicit what was vague in Meyer's development of the periodic systems.

Apart from satisfying Field's condition for successful definitions, the aptness of the definition of carefulness needs to go through Chang's test for the helpfulness of an abstract idea. As we saw, Chang stated that the cogency of abstract characterisations could be tested through further abstract considerations and arguments (Chang 2012b, 111). I suggest that within the project of clarifying the role of values in the context of development, the cogency of our abstract characterisations of values can be tested through contrasting how other related values fare at elucidating Meyer's approach. In what follows, I will argue that carefulness is more suitable than the related values accuracy (*Genauigkeit*), precision (*Präzision*), and sharpness (*Schärfe*) in making sense of Meyer's distinct approach to the systematisation of elements.

Sharpness

M. Norton Wise's edited volume (1995) brings together historical accounts on accuracy, precision, and exactness from a variety of contexts. In order to test the cogency of carefulness over other related values, in what follows, I will draw especially from Olesko's (1995) essay from the volume, where she details how sharpness, accuracy, and precision became part of refined measurements in the nineteenth-century German lands. Olesko's characterisation of respective values will allow us to compare and contrast them with carefulness, and test whether they fare better than carefulness in elucidating Meyer's approach to systematisation.

Among sharpness, precision, and accuracy, the latter two are more familiar. However, before accuracy and precision became the markers of refined measurements, sharpness (*Schärfe*, or *Schärfung*) was more often evoked as a characteristic of a successful experiment and investigator in the German lands (Olesko 1995, 110-111). Although 'accuracy' (*Genauigkeit)* and sharpness are at times difficult to distinguish from one another, Olesko highlights the following difference between the two: accuracy was associated especially with multiplication of observations, whereas sharpness described the

skills of the observer, and their verbal and visual descriptions of experiments and instruments used in investigations (Olesko 1995, 111).

The idea that sharpness characterised the personal qualities of the investigator becomes evident especially in Georg Muncke's account of "Beobachtung" in Gehler's *Physikalisches Wörterbuch*, published in 1825 (Olesko 1995, 107; Muncke 1825). Muncke thought it "useful and necessary" to know how the differences in "possible sharpness" that can be attained by "naked senses compared to those which instruments can yield" (Olesko 1995, 110-111). Such comparisons could be conducted if one was first aware of the quality of the instruments and their constant errors, where this could be achieved through sharp verbal descriptions and visual depictions of experiments and instruments (Olesko 1995, 111, 112-113).

Furthermore, apart from highlighting that sharpness through the reporting of experiments and instruments, Muncke also associated sharpness with the very character of the investigator (1995, 112). In Muncke's words, "the sharpness of the senses, the practice and patience of the observer or experimenter, even his state of mind, his perceived ideas and impulsiveness, indeed even accidents that can scarcely be determined" (Muncke in Olesko 1995, 112). As Olesko analyses, this was a source of personal authority of the investigator: "authority of measurement was not simply to be found in technologies – be they instruments or quantitative technology of least squares – but some authority was located in the personal qualities of the observer" (Olesko 1995, 112-113).

There are a number of reasons why sharpness, as characterised above, does not quite capture Meyer's approach to the systematisation of the chemical elements. As we saw, sharpness was with the observations of the investigator, descriptions of the trajectory of the experiment, and the workings of the instruments used for the experiment. However, Meyer did not detail his process of putting together his systems. Nor did he describe the instruments or the experiments made for attaining atomic weights and volumes. Although it remains plausible to characterise sections of Meyer's textbooks as sharp in the above sense, sharpness does not seem apt to characterise his approach to the systematisation of the elements. As we saw, the features that make Meyer's approach distinctive – as

summarised in table 6.1 – did not concern Meyer's qualities as an observer, or his descriptions of experiments, or his assessment of instruments. Instead, the features that made his approach distinctive concern his curation of the observations that gave rise to periodicity, and his assessment of the reported observations of others. For this reason, it would be best to discard sharpness as an analytic category to make sense of Meyer's approach to the systematisation of the elements.

Accuracy

As for accuracy, Olesko argues that it could be understood at least in three ways: description of measurements; the quality of laws based on observations; or the protocol of experiments (1995, 111). Crucially, irrespective of how the term was used, Olesko sees that "'the multiplication of observation' was the unconditional requirement for the highest possible accuracy" (Olesko 1995, 111). An accurate measurement was one that had been confirmed by several different observations.

The association of accuracy with the multiplication of observation raised the following question, though: how many times should observation be multiplied for gaining adequate accuracy? Many investigators turned to probability theory in answering this question, and by doing so, the investigators also ended up rendering the meaning of "Genauigkeit" as a distinctly probabilistic concept. For example, Gotthilf Hagen explained accuracy in terms of placing a bet, and J.J. Littrow defined accuracy in relation to the method of least squares, where the probability of one signified absolute accuracy (Olesko 1995, 111-112).

There is some textual evidence to support that Meyer valued accuracy in the sense of multiplication of observation. For example, in Outlines of Theoretical Chemistry, Meyer argued that "[t]he best guarantee against error of all kinds is secured when the atomic weight of an element has been determined by several distinct methods, and the results are found to agree" (1892, 50). Also, considering that Meyer took part in Franz Neumann's Königsberg seminar, where the method of least squares was a subject of intense interest, it is likely that Meyer, too, shared Hagen's and Littrow's understanding of accuracy in more probabilistic terms (Olesko 1991; 1995, 110).

While there is no explicit reference to accuracy or multiplication of observation in his discussion of the periodic systems, Meyer's discussion of the accuracy of Stas' atomic weights (Meyer 1864, 32; 1872, 34) implies that those chemical elements that were included in the system had been accurately determined in the sense that their atomic weights had been determined through multiple different methods. This suggests that accuracy was part of Meyer's approach to systematisation, although he did not explicitly refer to it in the course of explaining his systems.

However, I suggest we still refrain from using accuracy as the main category for analysing Meyer's approach to systematisation. Apart from the absence of explicit discussion of accuracy and multiplication of observations in relation to the systematisation of the elements, there is another compelling reason to resist using accuracy as the main category for analysing Meyer's approach to systematisation. Namely, in the modern literature on values in science, accuracy is rarely understood as multiplication of observation. Instead, it is firmly associated with truth or certainty. Philosophers of science would know to associate this view with scientific realism, the proponents of which maintain that the ultimate aim of science is truth, or approximate truth (Van Fraassen 1980, 8; Nola and Sankey 2007, 337; Khalifa 2018). The close link between accuracy and truth is also part and parcel of veritism, the view that takes accuracy as the ultimate epistemic value (Pettigrew 2016, 6; Goldman 2002, 52).

Employing an analytic category with such connotations to illuminate Meyer's distinct approach to systematisation would be too risky. This is because Meyer explicitly did not seek to establish truths about nature and did not express himself in terms of certainties. For example, in the introduction to his Outlines of Theoretical Chemistry, Meyer warned that "the best supported theory must never be regarded as absolutely true: a high degree of probability is the utmost to which it can attain" (Meyer 1892, 4). Slightly before this statement, Meyer issued a similar warning:

> A comparison of the theoretical deductions with the observed facts is the sole means of judging the correctness of the theory and of the hypothesis on which it is

> based. So long as facts and theory agree, we are justified in regarding the theory as accurate, but not as absolutely and infallibly true. (Meyer 1892, 3-4).

Although here the translator of Meyer's statement has chosen to phrase his message in terms of accuracy, Meyer's denial of theoretical chemistry being in the business of establishing what is "absolutely and infallibly true" implies that it is not the accuracy of the realist or veritist that Meyer intended here.

The above statements are from Meyer's 1890 discussion on the methodology of theoretical chemistry, but he also made similar warnings in earlier stages of his career. For example, in the conclusion of the second edition of his textbook, Meyer argued that:

> Such theoretical efforts, which often require as many arbitrary presuppositions as they are expected to explain facts, painfully recall the verdict with which, three-and-a-half centuries ago, Franz Baco of Verulam [Francis Bacon] struck our predecessors: "Chymicorum autem genus ex paucis experimentis fornacis philosophiam constituerunt phantasticam et ad pauca spectantem." Let us take care that this severe judgement does not remain applicable to us and our time; let's avoid superfluous and ill-founded hypotheses! *But let us be especially careful not to attribute to our best hypotheses a greater value than they deserve,* and to confound their yet so great probability with security and certainty! (1872, 363, emphasis added).[50]

This shows that it would be risky to use the term accuracy as the main analytic category in accounting for Meyer's approach to systematisation, as doing so would risk importing connotations on truth and certainty that would mischaracterise Meyer's position. However, there is at least one objection that the accuracy-valuing veritist or realist could bring up to counter my resistance. They could ask if Meyer's emphasis on avoidance of error could

[50] The Latin quote can be translated as follows: "There is, however, a kind of chemists who, from just a few experiments with a furnace, have constructed a philosophy which is both fantastical and of little relevance." I thank Nathan Gower for the translation.

stem precisely from his valuing of accuracy understood as closeness to truth as the ultimate aim for investigations?

The steps of this objection could be spelled out in more detail as follows. Recall that Meyer emphasised the importance of identifying errors (this was brought up briefly in chapter 4, and will be discussed more thoroughly in chapter 8). Now, if errors are taken as equivalent to inaccuracies, and avoiding of inaccuracies is taken as the flipside of valuing of accuracy, then it would be possible to push for the point that Meyer valued accuracy. From philosophical literature, this view of the relationship between accuracy and inaccuracy can be found both from the works of Richard Pettigrew and Kareem Khalifa. In his recent account on accuracy, Pettigrew states that "[t]he inaccuracy of a credence function at a world is just the negative of its accuracy, and vice versa … any claim stated in terms of accuracy can easily be translated into a claim about inaccuracy, and vice versa" (Pettigrew 2016,12). In a somewhat similar vein, Khalifa argues that inaccurate idealised and fictional models can be viewed as pursuing the broader aims towards accuracy. This is because they can be seen to take a "break" from the broader aim of accuracy specifically in order to pursue such an aim, just as a student whose goal is to learn might take a break from studying in order to enhance her overall aims (Khalifa 2018). Thus, we can interpret Meyer's warning against assuming that hypotheses or theories are certain and true as an expression of his more fundamental commitment towards accuracy.

There are at least two ways to contest the above objection. Firstly, we can dispute especially the premise of errors being interchangeable with inaccuracies. There are examples where errors distinctly do not indicate inaccuracy as distance from truth but are more closely associated with the relationship of one particular measurement with a group of measurements, which does not necessarily tell us anything about distance or closeness to truth. Legendre and Gauss' method of least squares gives us a good example of such understanding of error. Their method was based on the assumption that the true value of the measurement was unknown, and that only approximations of true value were possible (Olesko 1995, 106). Such approximations could be achieved by evaluating what the most probable values are. Such evaluations on the most probable values required estimating errors, where errors were "taken to be the difference between the measurement itself and

the average value of all measurements." (1995, 106). Thus, errors did not, in this case, signify distance from ultimate truth, but from the average value of all measurements.

Another counter-argument to the realist/veritist objection comes from the broader aims of this thesis. I have emphasised that my aim is an abstract characterisation of values which helps to elucidate their roles in concrete historical cases. However, arguing that Meyer nurtured an underlying second-order aim towards accuracy understood as the closeness of truth steps beyond what is suggested by the historical evidence. There is little evidence in favour of stating that Meyer ultimately aimed for ultimate truth when he explicitly discouraged us from thinking that even our best hypotheses and theories are true. The methodological stand taken in this thesis calls for a close connection between the historical case and the more abstract analyses, so pursuing accuracy as the analytic category would seem like loosening the connection between the historical evidence and the abstract analysis.

On these grounds, I suggest we should take Meyer's discussions on the avoidance of error to be independent from the realist/veritist conception of inaccuracy. Instead of linking accuracy with truth, a much more appropriate option is to view Meyer's approach as including accuracy in the more limited sense of multiplication of observations, which was needed to avoid accidental and constant errors. Furthermore, when we recall the indicators of carefulness summarised in table 6.1, it becomes clear that accuracy (understood as multiplication of observation) would be too narrow to capture the distinctness of Meyer's way of systematising the chemical elements. In sum, while accuracy understood as closeness to truth does not help in explicating Meyer's approach to systematisation of the elements, accuracy understood as the multiplication of observations is more present in his works (1864, 31-2; 1892,3-4, 50).

Precision

Similarly to accuracy understood as multiplication of observations, precision does not capture all the features in Meyer's approach to systematisation that render it distinctive. As detailed in chapter 4, the clearest indicator of precision was Meyer's use of several decimal places in reporting atomic weights of the elements. While employment of several

decimal points is an important component of Meyer's approach to classification, as I demonstrated, precision was only one of the many strategies Meyer employed.

Furthermore, Olesko argues that in the nineteenth century German lands, precision had a narrower meaning than it has now, and it was brought up more rarely than accuracy (Olesko 1995, 105). The term entered the German discussions through the method of least squares, and only gradually, over the course of the nineteenth century, became associated with extremely refined measurements obtained through instrumental means (Olesko 1995, 110).

Despite these reasons against using precision as the main category for analysing Meyer's approach, it is important to note how it is part of the understanding of carefulness introduced in this thesis. Juxtaposing indicators of precision with claims expressing uncertainty over results served as a means to show differences in data. In particular, a high degree of precision drew attention to cases where the quality of observations was outstanding, and it also helped to highlight where more work had to be done. Thus, the large number of decimals effectively showed the quality of observations that would then give rise for further observations on the relations between elements.[51]

To sum up this section, I have argued that Meyer's approach to systematisation is captured by carefulness better than with accuracy, precision, or sharpness. We saw that accuracy understood as a multiplication of observations was part of Meyer's views on observation and theory more broadly, but played only a smaller role in his explanations of his systematisations. However, precisely reported atomic weights helped to signal when the quality of observations was good and differentiate them from those that were lacking.

[51] In other contexts, the number of decimal points could signify the skills of the *investigator* in conducting experiments and assessing experimental results. As there is no evidence that Meyer engaged in atomic weight or volume determination in 1864-1872, it is more likely that Meyer used the data of others (where he mentioned Stas, and Roscoe, amongst others). Interestingly, Jan Golinski has brought attention to cases where such virtue-signalling strategies could also backfire – the results could be regarded as too good to be true! For example, when Antoine Lavoisier used several decimal numbers in reporting his measurements, a group of British chemists rejected his claims, as Lavoisier's language of precision appeared to them specifically as a *rhetoric* (Golinski 1995,74).

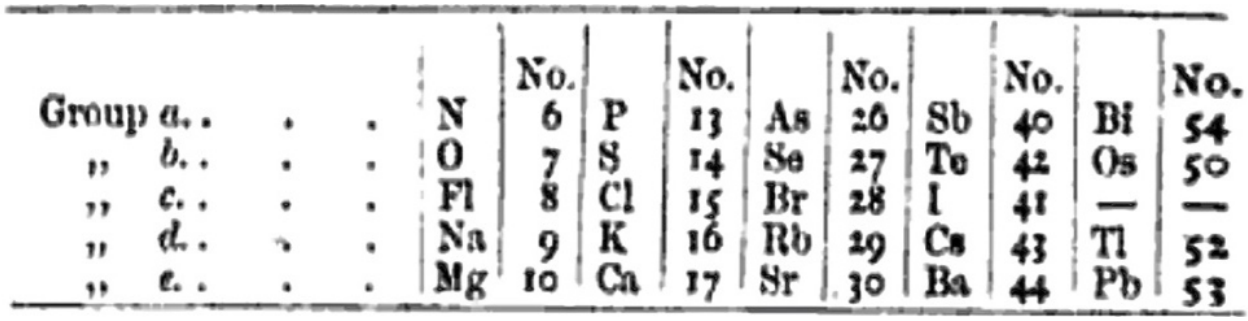
6.3 Simplicity

In this section, I argue that Newlands evoked different aspects of simplicity in different phases of developing his systems. Recall how in chapter 3, I broke down Newlands' process of developing his systems into three phases. In the first phase (1863-64), Newlands associated simplicity with regularity. In the second phase of development (1865-66), we saw that Newlands did not bring up the "simple relation." Despite this, I suggest that the aim for simplicity characterises Newlands' *design* of the Law of Octaves. In the third phase (1866), Newlands started bringing up simplicity again, but this time, he associated it with something simple in nature.

Simplicity of the First Phase of Development

Newlands' invoking of "simple relation" in the earliest stages of developing his system gives us an example of an actor's category which does not lend itself to a consistent abstract philosophical analysis. In what follows, I suggest that two initially promising ways of analysing simplicity of 1863-1864 – simplicity as regularity and simplicity as understanding – unfortunately do not allow for a consistent abstract analysis of simplicity of Newlands' early systems.

Before demonstrating this, it would be helpful to start with a brief reminder of how Newlands associated simplicity with the regularity of a numerical relation. In chapter 3, I noted that in the earliest stages of developing his tables, Newlands highlighted having found a "simple relation" among the elements. And, indeed, when examining the resembling elements of the natural groups in Newlands' paper of 1863, there appears to be an interesting regularity: the numerical difference between the element with the lowest equivalent and the one preceding is about 8 or 16. This finding Newlands associated with triads, as he noted that a "similar relation" could be shown by "deducting the lowest member of a triad from the highest" (1863, 71). Thus, at this stage, Newlands linked the simplicity of his arrangement with the numerical regularity that arose among similar elements.



Group	*a*. .	.	.	N	No. 6	P	No. 13	As	No. 26	Sb	No. 40	Bi	No. 54
,,	*b*. .	.	.	O	7	S	14	Se	27	Te	42	Os	50
,,	*c*. .	.	.	Fl	8	Cl	15	Br	28	I	41	—	—
,,	*d*. .	.	.	Na	9	K	16	Rb	29	Cs	43	Tl	52
,,	*e*. .	.	.	Mg	10	Ca	17	Sr	30	Ba	44	Pb	53

Figure 6.2 Newlands' table of 1864.

Newlands continued doing so in his following publications, too. In 1864, Newlands illustrated regular numerical relationships with a table (figure 6.2). In a note attached to the article, Newlands stated that the atomic weight of indium would bear a "simple relation" to zinc group elements. In particular, he suggested that its atomic weight would be close to zinc or cadmium (1864c, 95). Like Newlands' previous statements on the "simple relation," the comment on indium, too, implies that Newlands associated simplicity with the regular numerical relation among analogous elements.[52]

Newlands' comments in both settings imply that he associated the "simple relation" with the regular numerical relation found amongst similar elements. This suggests that when giving a more abstract characterisation of the simplicity of Newlands' system, we should attempt to analyse it in terms of regularity. However, existing accounts of simplicity link it with minimisation rather than regularity (Baker 2016). For example, authors who consider ontological simplicity (or parsimony), associate simplicity with the minimisation of entities (Sober 2015; Baker 2016; Schindler 2018, 13-14). In contrast, the simplicity of theories (also known as syntactic simplicity) concerns the minimisation of posits, principles, or laws (Currie 2018; Schindler 2018, 13). Thus, in order to analyse simplicity with regularity, we would have to find out what about regularity concerns minimisation.

Unfortunately, there seems to be little about regularity per se that speaks of the simplicity of relations. After all, we may plausibly conceive of a highly regular relation that is also

[52] Although it should be noted that Newlands' suggestion that indium would bear a "simple relation" to zinc group, i.e., Mg=10, Ca=17, Sr=30, does not make it clear where it could be fitted on the table.

incredibly complex. If we were to analyse simplicity in terms of regularity, we would then have to argue why complex yet regular relations are simple. One option would be to say that regularities may appear simple in comparative terms; the regular yet complex relation can appear simple when we contrast it with alternative irregular relations. Thus, in this solution, Newlands' "simple relation" is simple because it appeared so in contrast with other ways to organise the elements. In short, the "simple relation" is simple because it is more regular than what other systems conveyed.

Unfortunately, this analysis of simplicity remains somewhat unconvincing. Firstly, Newlands did not argue that his systems were more simple than the other ones in the market. Instead, Newlands' comments imply that the simple relation appeared simple on its own. However, even if we were to push for the point that Newlands' judgement of simplicity was made on comparative grounds, it is difficult to see how Newlands' groups were more simple than systems which distinguished between, say, metals and metalloids. After all, the division between metals and metalloids operated with fewer organising principles than Newlands' arrangement; they just divided them up according to metallicity. Thus, the fact that regularity can be independent of simplicity implies that there is little in regularity per se that would speak of simplicity. How, then, are we to make sense of Newlands' statements of 1863-1864?

There is an alternative solution: we could argue that the regular numerical relationship amongst "allied" elements appeared simple because it provided a sense of understanding. When looking at the tables, one could grasp that the regular quantitative atomic weight data was compatible with showcasing the qualitatively similar groups. However, equating simplicity with the feeling of understanding raises the following question: what marks the understanding provided by other classifications as different from the understanding provided by Newlands' early systems? The proponent of analysing simplicity in terms of understanding could now say that the groupings between metals and metalloids – although simple in their organization – concealed some dissimilarities between the elements. For example, the class of metals could incorporate elements that were more similar to the elements grouped with non-metals, although they exhibited the properties of metallicity. In short, while such classifications based on simple divisions could provide a

sense of understanding, they could also appear confusing, and raise difficult questions about membership. The proponent of analyzing simplicity in terms of understanding could now point out that Newlands' early tables did not suffer from this problem to the same extent.

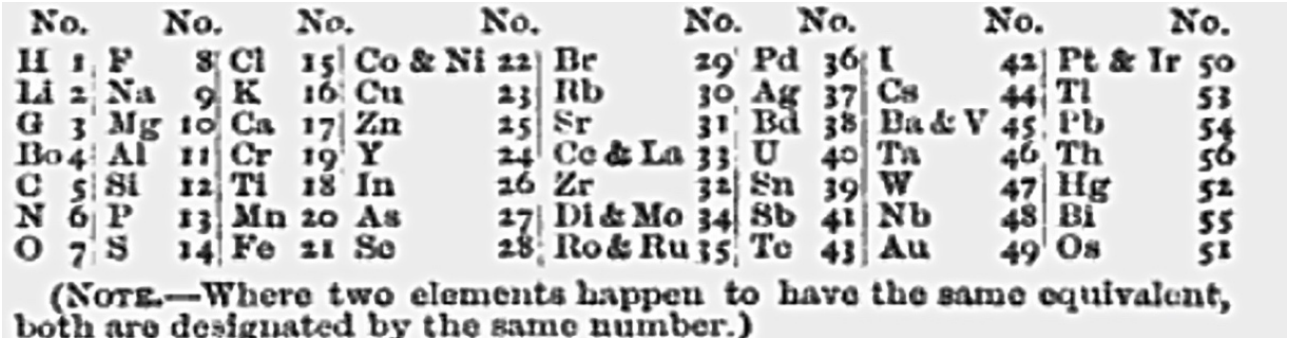
However, in some cases, the groups of that table of 1864 also included elements whose membership we could dispute. For example, the group *b* (O, S, Se, Te, and Os) included osmium (Os), which in modern terms is an actinoid, and not grouped with oxygen, sulfur, selenium, and tellurium. However, including osmium is not just contestable from a modern point of view. For example, the natural group of elements included in Watts' chemical dictionary (see figure 2.1 in chapter 2) ruled out osmium from the group of oxygen, sulfur, selenium, and tellurium. Furthermore, Meyer's tabular periodic systems of 1864, 1868, and 1870 placed it elsewhere. In a similar vein, Newlands's group *d* in the table of 1864 included thallium (Tl) alongside the alkali metals (Na, K, Rb, and Cs). In modern terms, the abovementioned alkali metals are in group 1 of the periodic table, whereas thallium can be found in the post-transition metal group. In the 1860s, Mendeleev placed it quite far from the alkali metals (see e.g. figure 5.6 in chapter 5).

Thus, we can dispute the claim that Newlands' systems provided a greater sense of understanding because they displayed similarities between the elements. As we will see, in the next iterations of the system, Newlands adhered even less to the idea that elements with regular numerical relations should always be similar to one another, which pulls the rug under the project of trying to cash out the simplicity of the system in terms of how it gives us a sense of understanding.

The second phase of simplicity

Although Newlands' "simple relation" of 1863-1864 does not allow for a consistent abstract analysis of simplicity, his systems *appear* distinctly simple nevertheless (see figures 6.2, 6.3, and 6.4). I suggest that analysing the simple appearance of the systems of 1864, 1865 and 1866 gives more fertile grounds for abstract philosophical analysis of simplicity. In particular, I will argue that Adrian Currie's articulation of theoretical simplicity – when slightly modified – allows us to spell out the simplicity of the systems.

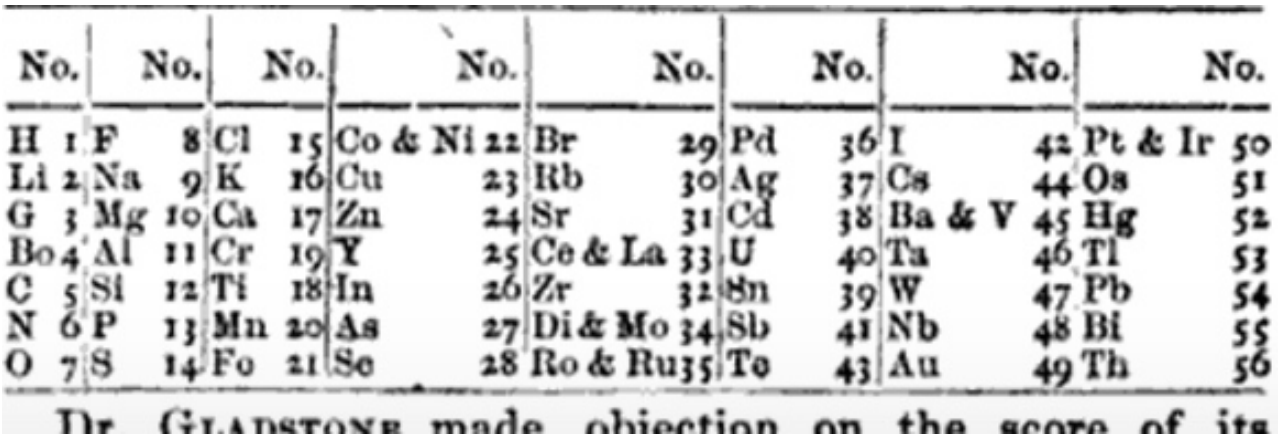
Currie introduces the value of theoretical simplicity in his discussion of the simplicity of paleobiological explanations. Similarly to the earlier accounts on simplicity, Currie's view on theoretical simplicity involves minimisation. In particular, he suggests that it concerns the minimisation of "number of variables and posits in a theoretical structure" (2018, 3). Currie notes that theoretical simplicity can go hand-in-hand with ontological simplicity insofar as the phenomena the theory seeks to represent are simple. However, Currie also points out that at times, theories can appear simple because they have been constructed to look simple, rather than because of the simplicity of phenomena.



No.		No.		No.		No.		No.		No.		No.		No.	
H	1	F	8	Cl	15	Co & Ni	22	Br	29	Pd	36	I	42	Pt & Ir	50
Li	2	Na	9	K	16	Cu	23	Rb	30	Ag	37	Cs	44	Tl	53
G	3	Mg	10	Ca	17	Zn	25	Sr	31	Bd	38	Ba & V	45	Pb	54
Bo	4	Al	11	Cr	19	Y	24	Ce & La	33	U	40	Ta	46	Th	56
C	5	Si	12	Ti	18	In	26	Zr	32	Sn	39	W	47	Hg	52
N	6	P	13	Mn	20	As	27	Di & Mo	34	Sb	41	Nb	48	Bi	55
O	7	S	14	Fe	21	Se	28	Ro & Ru	35	Te	43	Au	49	Os	51

(NOTE.—Where two elements happen to have the same equivalent, both are designated by the same number.)

Figure 6.3 Newlands' system of 1865 (1865, 83)



No.		No.		No.		No.		No.		No.		No.		No.	
H	1	F	8	Cl	15	Co & Ni	22	Br	29	Pd	36	I	42	Pt & Ir	50
Li	2	Na	9	K	16	Cu	23	Rb	30	Ag	37	Cs	44	Os	51
G	3	Mg	10	Ca	17	Zn	24	Sr	31	Cd	38	Ba & V	45	Hg	52
Bo	4	Al	11	Cr	19	Y	25	Ce & La	33	U	40	Ta	46	Tl	53
C	5	Si	12	Ti	18	In	26	Zr	32	Sn	39	W	47	Pb	54
N	6	P	13	Mn	20	As	27	Di & Mo	34	Sb	41	Nb	48	Bi	55
O	7	S	14	Fe	21	Se	28	Ro & Ru	35	Te	43	Au	49	Th	56

Dr. GLADSTONE made objection on the score of its

Figure 6.4 Newlands' system of 1866 (Anon 1866, 113)

In what follows, I suggest that Currie's articulation of theoretical simplicity offers a fruitful avenue for a more abstract characterisation of the simplicity of Newlands' Law of Octaves (1865-1866). Applying the category of *theoretical* simplicity requires some motivation, though, because the Law of Octaves (and periodic systems more broadly) can be characterised both as classifications *and* theoretical arrangements. As to the theoretical nature of Newlands' systems, recall that Newlands' contemporaries perceived his system as distinctly theoretical (see section 3.3 of chapter 3). As we saw, especially Gordin (2018) emphasised that its reception was influenced by its theoretical appearance. However, apart from the distinctly theoretical nature of the systems, it is important to appreciate that they were not just theories, but also tabular classifications. Although classifications in science can take a variety of shapes and forms, in this context, the classificatory nature of the systems is exemplified by the fact that they were distinctly tabular depictions of the elements.

The merging between classificatory and theoretical nature of the tables has consequences for making sense of their simplicity. Although we may analyse their simplicity in terms of how many postulates or variables they have, we should also take into account the number and complexity of the parameters that guided the organisation of the classifications. As noted in chapter 2, for the early periodic systems, there were at least the following two parameters: representation of the qualitatively similar groups and the linear quantitative ordering according to the magnitude of atomic weight.

The fact that the periodic systems are simultaneously classifications and theoretical arrangements calls for making slight amendments to Currie's definition of theoretical simplicity. To render the account especially helpful at articulating the simplicity of Law of Octaves, I suggest we best broaden Currie's definition to make sense of the simplicity-inducing design choices involved in the construction of classifications. In other words, for encompassing the full variety of scientific representations, it is not enough to account for the number of variables and assumptions made for theories. Instead, our definition of theoretical simplicity has to encompass the design-choices involved in constructing theoretical representations of all shapes and forms. For this reason, I suggest the following amendment to Currie's definition of simplicity:

> Theoretical simplicity concerns the minimisation of number of variables, posits, organisation parameters and visual properties in a theoretical structure.

This amended definition still allows us to state why a theory that would posit three fundamental laws of motion is simpler than a theory postulating five fundamental laws of motion (e.g., Schindler 2018, 14). However, as we will see, the amendments also make the definition useful for pinpointing what makes some visual representations – such as periodic systems – simple.

In particular, Currie's amended definition helps to state the simplicity of Newlands' systems in more abstract terms for four reasons. Firstly, it highlights that the simplicity of Law of Octaves stemmed from its features as a *representation* of phenomena, rather than the phenomena itself. Especially relevant here is Newlands' use of the order numbers. As we saw, they rendered the regular numerical relation a feature of Newlands' design choices. Especially Gordin has emphasised the theoretical nature of the Law of Octaves, as he stated that there was "no indication that this [Law of Octaves] is more than a fortuitous coincidence at best, and an artefact of faulty atomic-weight determinations at worst. There is, in short, no data-theory balance … and therefore nothing to vouchsafe the regularity" (Gordin 2018, 38). The theoretical appearance of the Law of Octaves implies that when we attempt to explicate its simplicity, it would be more fruitful look for its source from the features of it as a *representation* of phenomena, rather than the features of the phenomena itself.

The second reason why the updated version of Currie's theoretical simplicity is apt for analysing the simplicity of the Law of Octaves concerns the use of order numbers. Not only did they make Newlands' arrangement appear more theoretical, but they also helped to minimise the quantity of numbers used to express atomic weights. Furthermore, the order numbers also allowed for a clearer expression of the numerical regularity than the standard atomic weights did.

Thirdly, something that speaks of the fact that Newlands was in the business of minimising is that Newlands cut one of the original organising parameters (i.e. reserving one horizontal line of the system for one group of resembling elements) from the 1865 and 1866 versions of Law of Octaves. This becomes clear when we compare the systems of 1864 and 1865. By 1865, no longer was it important just to dedicate one horizontal line for one qualitatively similar group. Instead, the lucid expression of the numerical relationship itself became a higher priority. This contrast suggests that Newlands had compromised one of the parameters that guided his organisation of 1864 and allowed for the groups to be *found* from the horizontal lines instead of dedicating the whole horizontal line for qualitatively similar groups. Thus, in comparison with the previous system of 1863-1864, it is clear that Newlands had cut one of the organising parameters.

Finally, apart from Newlands's wielding of the razor to cut one of the original organising parameters, another telling sign of the system's aim to minimise visual properties is that Newlands resisted adding more vacancies to the system in 1865-1866. Instead of adding new spaces, Newlands found it more attractive to place several elements to the same position, even at the prospect of the quantity of elements exceeding the number of the slots available.[53] Giving up one of the original organising parameters, and resisting the addition of places for each individual element shows that Newlands sought to minimise the parameters and visual properties of his system; doing so allowed him to express the Law of Octaves rather than a Law of Several Musical Intervals. Newlands' resistance to adding more vacancies to the system in 1865-1866 speaks in favour of his aims to minimise the visual properties of the system.

Simplicity and the Third Phase of 1866-1875

As the "simple relation" of 1865-1866 was primarily associated with the system's appearance, Newlands' later statements on how his system *identified* a simple relation or a law were somewhat unconvincing. For example, when presenting his system of 1866 to Chemical Society, Newlands stated that:

[53] In 1875, when Newlands issued a new system, where he extended the number of groups to sixteen.

> I have endeavoured to describe *relations actually subsisting among the atomic weights of the elements at present known*, but am far from thinking that the discovery of the new elements (or the revision of the atomic weights of those already known) will upset, for any length of time, the existence of a *simple relation* among the elements … The fact that such *a simple relation* exists now, affords a strong presumptive proof that it will always continue to exist, even should hundreds of new elements be discovered. For, although the difference in the numbers of analogous elements might, in that case, be altered … the existence of a *simple relation* among the numbers of analogous elements would be none the less evident. (Newlands 1866, 130).

Newlands' response illustrates how the alleged simplicity provided evidence for successful identification of a relation amongst resembling elements. As we saw in chapter 3, Newlands continued this line of argumentation in his later writings. In 1872, after growing awareness of Meyer and Mendeleev's systems, Newlands asserted his priority on the discovery and stated that "it was only with the atomic weights of Cannizzaro that such *extremely simple relationship* could be observed" (Newlands 1872 reprinted in 1884, 21 emphasis added). Furthermore, in chapter 3, we also saw that Newlands emphasised simplicity even more strongly in the 1884 book that compiled together different versions of the system. In that context, Newlands argued that already in 1864, he had "announced the existence of *a simple relation or law* among the elements when arranged in the natural order of their atomic weights" (1884, vi, emphasis added). Immediately after making this statement, he reiterated the simplicity of his system of 1865 by stating that in 1865 "In the Chemical News … I published a full horizontal arrangement of the elements in order of atomic weights, and proposed to designate *the simple relation* existing between them by the provisional term 'Law of Octaves'" (1884, vi, emphasis added). He concluded with suggesting that he had been "the first to publish a list of the elements in order of their atomic weight, and also the first to describe the periodic law, showing the existence of a simple relation between them when so arranged" (1884, vii).

What unites these statements of 1866, 1872, and 1884 is Newlands' idea that the simple relation of 1865 spoke of a pattern in nature. However, Newlands' claim that his system

effectively demonstrated the existence of (a simple) relation among analogous elements remains somewhat unconvincing for two reasons. First, Newlands altered the systems of 1865 and 1866 so that it was not easy to discern between relevant and irrelevant analogies like on the system of 1864. As emphasised in chapter 3, analogies could be *found* from the horizontal lines, but it took a trained eye to identify them amongst the elements. After all, some of the elements that formed regular relations were not marked as analogous in Newlands' earlier natural groupings.

Second, contrary to what Newlands claimed, it is unlikely that the entrance of new elements would have retained the simple relation. As noted in chapter 3, the difference between elements on the horizontal lines of systems of 1865-1866 was usually 7. However, the accommodation of a new element with highly different atomic weight would have been difficult without disturbing the regularity in the numerical differences or the neatness of the arrangement more generally. This is because Newlands only allowed for double-bookings for elements with close atomic weights.

In conclusion, I suggest Newlands' systems do not allow for a consistent abstract philosophical analysis of simplicity. This is because either his comments did not make it clear what exactly contributed to the simplicity of the "simple relation," or the factors that seemed to most clearly speak of the simplicity of his arrangement (as discussed in the section on theoretical simplicity) were in tension with Newlands' claim that the simplicity spoke of an identification of a pattern in nature. In particular, I sought to demonstrate that the most consistent analysis of the simplicity of the systems arose from their design features. In other words, its simplicity appeared to be a property of the design of the classification rather than a property of the phenomena. This source of its simplicity rendered Newlands' later claims about simplicity speaking in favour the relation between atomic weights "actually subsisting" somewhat unconvincing.

As such, Newlands' case gives us an interesting lesson for iHPS investigations into values in science. Although philosophers might be inclined to seek consistency in actors' values, our interpretation of values needs to reflect both the inconsistencies and consistencies in how actors valued qualities.

6.4 Completeness

Unlike with simplicity or carefulness, we cannot compare several different conceptions of completeness from the literature and test the cogency of one interpretation of completeness over others. In contrast with other values examined in this thesis, completeness has not received quite so much attention in the literature on values. Instead of testing the cogency of the definition of completeness through comparing with different definitions, I will take one of the most substantial accounts on the literature – Chang's definition of completeness, as introduced in his account of the Chemical Revolution – and contextualise it to reflect specific concerns of systematisation of the chemical elements in the 1860s. After doing so, I will show how it can be formulated to make sense of both the completeness of Mendeleev's and Newlands' systems, and the (in)completeness of Meyer's. However, I will argue that unlike Newlands and Meyer, Mendeleev's emphasis on completeness included both qualitative similarities and the number of elements. Thus, where we can state that Newlands aimed for quantitative completeness (i.e. high number of elements), and Meyer for qualitative completeness (i.e. high number of qualities), Mendeleev kept both on board.

In his treatment of phlogistonist and oxygenist accounts of combustion, Chang argues that Joseph Priestley's account of combustion differed from the oxygenist account of Lavoisier in various different ways. One of the differences concerned values. Where Lavoisier's account emphasised simplicity, Priestley's was more complete. In order to highlight this difference, Chang defines completeness as "wanting to account for all the observed phenomena in a given problem area and for all the observed aspects of those phenomena" (2012a, 22). By introducing the notion of a problem area, Chang's definition makes completeness attainable, as it only calls for completeness with respect to a particular problem at hand. This marks it as different from understanding completeness as a global aim for science (Hoyningen-Huene 2013, 36–37, 125–134).

Both the idea that completeness can be achieved within a specific task and the fact that Chang introduces the definition to capture completeness in a specific episode in history of chemistry render this definition a good starting point. However, specific concerns in

systematising elements suggest that the definition is simultaneously both too narrow and too demanding.

Let's first consider how it might be too demanding. By the 1860s, the amount of information concerning elements and their compounds was considerable. The plenitude of information posed an obvious challenge for teachers of chemistry, who had to search for ways of introducing the many findings of chemistry to their students. In the words of Bensaude-Vincent, the chemistry teacher "was compelled to teach a long and arduous list of monographs describing each of the known elements. The endless growing population of elements seemed to condemn chemistry to something like a natural history of chemical substances" (Bensaude-Vincent 1986,5). As noted in chapter 2, many of the classifications that were later recognised as precursors to the periodic system were formulated as a solution to this pedagogical problem.

Considering the multitude of information on the chemical elements and their compounds, it would be overly demanding to expect that a classification or a systematisation accounted for every observed phenomenon and all of its observed aspects. Thus, instead of having to account for everything, I suggest that it is enough for completeness to only account for those features that chemists themselves deemed as the most crucial or relevant. For example, in 1863, Newlands explicitly stated that chemical features guided his first groupings over physical features (1863, 70). Thus, insofar as Newlands' system achieved this end, we could credit it with completeness (but, as we saw, this was not the case).

Apart from making completeness less demanding, the specific concerns arising from systematisation in the 1860s also suggest that it should be made less narrow. Recall that Chang suggested that completeness involved accounting for observed phenomena in a problem area. While this is quite clear, at times, chemists expressed that also phenomena that were likely to enter the problem area needed accounting. This was especially the case with chemical elements. As we saw, by the end of the 1860s, chemists expected that new elements would be discovered. For example, when Newlands presented his classification of the elements to the Chemical Society, one of the objections was that his arrangement did not make it clear how new elements might be fitted.

The possibility of new elements entering the problem area suggests the following modification to our definition of completeness: it is not enough just to say something about observed phenomena and their aspects, but we also need to say something about phenomena that are expected to enter the problem area. On these grounds, I suggest the following definition of completeness:

> *Valuing of completeness involves wanting to account for as many phenomena and their observed aspects as possible in a given problem area, giving special attention to phenomena that are deemed most relevant, and saying something about accommodating phenomena that are likely to enter the problem area.*

Now that we are armed with this definition, it becomes easier to see how chemists emphasised completeness. As I argued in chapter 3, by 1864, Newlands associated completeness with a high number of the elements included in the systems. However, especially his systematisations of 1865 and 1866 fell short in accounting for many of their observed aspects, as the qualitative similarities were not that important a message for the system to convey. However, it is also clear that Newlands' system was also not very complete in the sense that it did not make clear where to place elements that were likely to enter the problem area. In order to highlight that for Newlands completeness primarily concerned the number of elements included, I will refer to it as *quantitative completeness.*

With Meyer, we saw that especially his discussion of the atomic volume graph brought together various qualitative properties of the elements. Thus, while Meyer did not account for quite so many elements, he sought to account for many of the observed aspects of those phenomena that he had included in the system. Furthermore, both the graph and the systems suggested places for undiscovered elements. We might distinguish Meyer's sense of completeness from that of Newlands by referring to it as *qualitative completeness.*

Where Newlands' systems emphasised quantitative completeness and Meyer's systems qualitative completeness, a closer examination of Mendeleev's system shows that he

emphasised completeness in both qualitative and quantitative senses. In chapter 5, I brought attention to the following six features that speak of valuing of completeness in Mendeleev's publications: (1) inclusion of 63 elements in the system, (2) considering at least seventeen chemical and physical properties in relation to his system, (3) emphasising the importance of completeness (*polnost'*) for a systematisation, (4) suggesting places for undiscovered elements, (5) incorporating more information (e.g. the row of oxides) to the visual layout of the system, and (6) evaluating the previous systematisation attempts in terms of completeness. In what follows, I will outline how this more abstract definition of completeness captures the above features of Mendeleev's approach.

As noted in chapter 5, Mendeleev's aim for completeness was indicated by his very first attempt of classifying the elements. If we compare this system with those of Newlands, Odling, and Meyer, we notice that Mendeleev included a higher number of elements. Meyer's earliest tables in conjunction had 50 elements, albeit its purpose was not to classify all of them. The fairer points of comparison are the tables of 1868 and 1870, but they too show a difference. By 1868, Meyer's number of elements rose to 53, by 1870 to 55, and finally, the table of 1872 included 56. Newlands's Law of Octaves had 62 elements in 1865 and Odling included 57 elements in 1864 (Odling 1864). The earliest version of Mendeleev's system had 63 elements.

In chapter 5, we also saw how (2) Mendeleev brought in a large number of chemical properties in his discussion of how resembling elements were distributed in the system. The third indicator (3) also highlights that he emphasised how most of the properties of elements should be kept in sight. In chapter 5, we saw that while Mendeleev dismissed physical properties (such as density) and chemical properties (such as acidity and basicity) as organizing principles, he also maintained that they ought to be kept in sight during classification. Furthermore, while Mendeleev disregarded chemical properties as an unsuitable organizing principle, he still maintained that "they [chemical properties] should not be left out of sight, because many aspects of chemical findings are generalized with their help" (1871c, 104).

We also saw how Mendeleev's call for including a larger variety of features was not just a recommendation, but that (4) Mendeleev's systems in fact left plenty of ways to accommodate newcomers. Apart from numerous gaps he also allowed the groups on vertical columns remain uneven, thus making space for elements that would be discovered in the future. This feature also provides us an answer to why Newlands's system appeared less complete than the system of Mendeleev; it did not make it clear how to accommodate new chemical elements. While Newlands had been complete with accounting for the known chemical elements, he did not make it explicit how to place the undiscovered ones, or emphasise how chemical similarities were distributed to the system.

Furthermore, when examining the systems, we can see that Mendeleev (5) in fact brought in many observed aspects of the elements into the system. This becomes clear when we compare Mendeleev's different systems with one another. For example, it is visible that the system that was included in *Principles of Chemistry,* Mendeleev had added formulae of compounds and the schematic rows of oxides and hydrides. Mendeleev introduced these rows first in 1870 and included them in most of the subsequent versions of the system. From the chemists classifying the elements in 1860-1870, Mendeleev was the only one to include them visually in the system.

Finally, the sixth indicator (6) highlights that completeness was also a normative requirement for Mendeleev. For instance, after surveying previous attempts of classifying the elements, Mendeleev argued that the groupings of Peter Kremers offered only "fragmentary findings that did not lead to a complete systematisation" (1869b, 15). Moreover, when Mendeleev was considering different organizing principles for his systematization, he pushed away physical properties as organizing principles on the grounds that their study "(…) are not yet in that level of precision and generality that would allow for creating *a complete scientific system.*" (1871c, 103, emphasis added).

From these six indicators, some (1&4) more clearly contribute to quantitative completeness, and others (2) are more firmly tied to the qualitative properties of elements. Furthermore, unlike with Newlands and Meyer, Mendeleev emphasised how systematisations more broadly should aim for completeness (6). This suggests that

valuing of completeness was present throughout the development of periodic systems both in Mendeleev's *rhetoric* of wanting to account for many observed phenomena, and his *practice* of giving attention to a large variety of observed phenomena in the course of systematisation. As we saw, this was not only limited to the known elements, but also suggesting the properties and places of unknown elements.

6.5 Conclusion

In this chapter, I have put forward a more abstract analysis of simplicity, completeness, and carefulness as an interpretation of the concrete instances of valuing present in systematisation of the elements. The detailed account of specific values paves the way for the next task of this thesis – the examination how the values related to the chemists' later applications of the systems. As hinted at in the historical sections, the systems did not *solely* classify the elements. Especially Mendeleev and Meyer used their system for further purposes. (In 1884, Newlands provided a list of applications for Law of Octaves, but most of them were *post hoc*). The applications of the systems included making generalisations, describing properties of little known and unknown elements, and identifying errors in empirical results. From the many uses of the systems, chapters 7 and 8 focus on two – predicting the properties of the elements, and identifying errors in empirical results. More specifically, chapter 7 examines how Mendeleev's valuing of completeness supported using the system for detailed predictions of unknown and little-known elements. Chapter 8 articulates how valuing of carefulness during the development of Meyer's periodic systems allowed Meyer to use the system as a diagnostic tool to identify errors in experimental results.

Chapter 7. Completeness and Predictions

7.1 Introduction

The fact that Mendeleev's periodic system is so often remembered for its accurate predictions is a question of values. In particular, it seems to imply that there is something about his predictions that we find appealing, virtuous, or convincing.[54] The valuing of predictive accuracy is reflected in many historical and philosophical accounts of the periodic system, as a considerable number of authors devote at least some of their attention to detailing (or even praising) the accuracy of Mendeleev's predictions of the eka-elements (Karpenko 1980, 79; Lipton 1990; Brush 1996; Brush 2007; Barnes 2008; Kaji, Kragh, and Palló 2015). Most recently, the United Nations General Assembly and UNESCO declared 2019 as the year of the periodic table, where their publicity material draws attention to how the system has been used for making predictions (IYPT, n.d). Their logo for the year does not show Newlands, Meyer, Odling, or Chancourtois, but only Mendeleev (see figure 7.1).

Figure 7.1 IUPAC's logo for International Year of the Periodic Table of the Chemical Elements

[54] See Lipton (2005) for why this might be the case.

As noted in the introduction to this thesis, my focus here has been predominantly on the process of developing of the systems. The processes of development were mostly free from considerations of predictive accuracy because the accuracy of the predictions of the chemists could only be judged many years after the completion of the development of the systems. In Mendeleev's case, for example, the accuracy of his predictions could be determined only after 1875, when other chemists started finding substances that were similar to Mendeleev's predictions of 1871.

By 1875, Mendeleev had virtually stopped working on the periodic system. This shows that predictive accuracy was not as relevant to the development of the system as it was for its reception (see especially Brush 1996; Kaji, Kragh, and Palló 2015). However, while predictive accuracy is not something that arises during Mendeleev's development of the system, the same cannot be said of making predictions. Indeed, in 1871, Mendeleev started bringing attention to how the system – and the periodic law – could be used for making detailed predictions of little-known and unknown elements. Making such predictions is different from predictive accuracy, as, in 1871, it was unclear whether they would come true or not.

It is this sense of *making* predictions that will be the focus of this chapter. Such a viewpoint of Mendeleev's predictions is partially a consequence of the framing of this thesis, where I have placed the questions of development at the forefront and downplayed reception or the narrowly construed context of justification (Schickore and Steinle 2006). However, pushing predictive accuracy off its pedestal to make room for prediction as an application of the system is not just a methodological point. Such framing of Mendeleev's predictions also reflects how Mendeleev himself characterised his predictions in 1871. The accuracy of the predictions could not be part of his narrative like it was in his later speeches and statements. Instead, when we look at making predictions instead of predictive accuracy, they appear as bold, risky claims about how to apply the "law of periodicity" to describe the properties of unknown elements. Furthermore, so far there have been no detailed narratives of *how* Mendeleev issued his predictions, so a more detailed description should be a welcomed addition to the literature (see Gordin 2004, 36-38 for a very brief sketch on how Mendeleev made his predictions). Although takes on the value of prediction (versus

accommodation of evidence) are plentiful in the literature (Lipton 1990; Scerri and Worrall 2001; Barnes 2008; Schindler 2014; Woody 2014) reports on how Mendeleev arrived at his predictions are scarce (see Woody 2014, 134 for a denial that Mendeleev used the periodic system for making predictions).

Apart from supplementing the literature with an account of how Mendeleev issued his predictions, this chapter also establishes a relationship between his valuing of completeness and detailed predictions of 1871. In particular, I suggest that the valuing of completeness during the development of the system supported Mendeleev's use of the system to make detailed predictions of the properties of little-known and unknown elements.[55] As we will see, completeness supported both Mendeleev's predictions of 1871 that are deemed "successful" (indium, eka-boron) and "unsuccessful" (didymium). However, in contrast, his later predictions of 1904 (ether, coronium) did not make use of the system's completeness in accounting for observed chemical and physical phenomena. This is because Mendeleev assumed that coronium and ether were inert and extremely light. Thus, by default, he could not draw on his rich pool of knowledge of the behaviour of other elements in his predictions of the properties of coronium and ether like he had done in 1871.

However, before I can demonstrate how completeness supported *making* predictions in this manner, it is important to appreciate that Mendeleev (and other chemists) did not just make one kind of prediction. Those acts of foreseeing which have retrospectively been titled as predictions could vary from a gap on a table to a detailed description of the chemical properties of ether. Indeed, such a variety of different kinds of predictions can be observed in Mendeleev's works alone. For this reason, before setting to show how completeness supported predicting, a finer taxonomy of predictions would be helpful.

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[55] Mendeleev's seminal paper did not only focus on predictions as the application of the system (Mendeleev 1871c). The paper includes four lengthy sections which are characterised as applications of the periodic law. As noted in chapter 5, some of them were *post hoc,* and I have excluded them from the discussion of this section of the thesis. Similarly, I have excluded Newlands's 1884 list of applications of his law, as most of them were similarly *post hoc* and there is little to no evidence that Newlands in fact used the system in such manner in his earlier publications on the Law of Octaves.

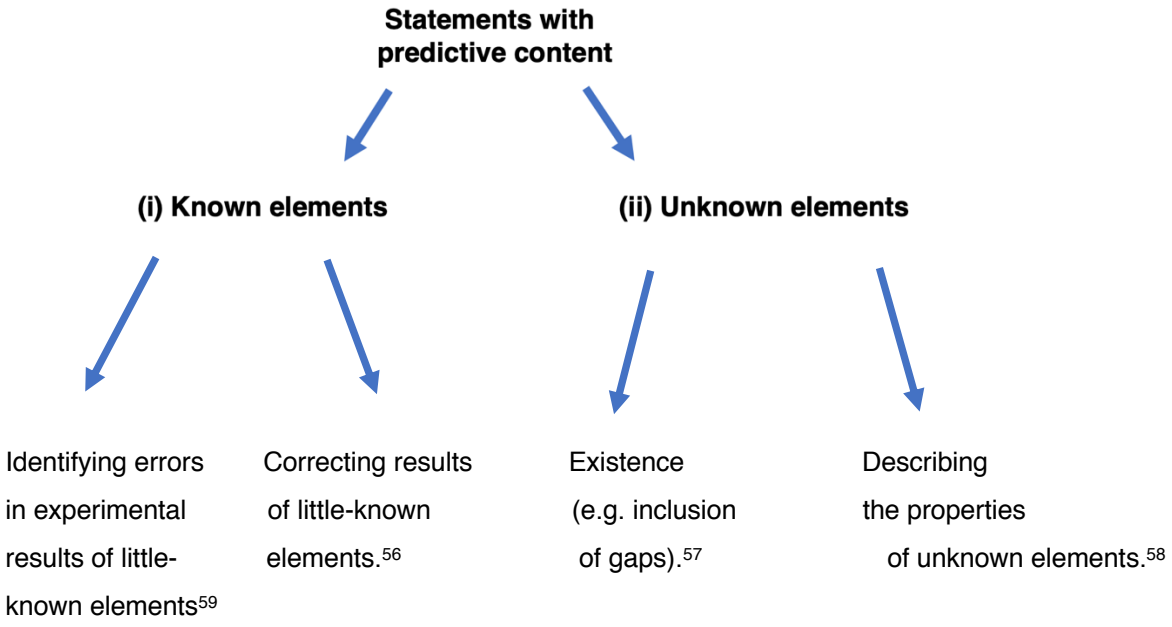


Figure 7.2 Taxonomy of chemists' statements with predictive content

In figure 7.2, I propose one way of distinguishing between different kinds of predictions found in the publications of chemists examined in this thesis. As can be seen, the taxonomy does not focus on 'predictions' but 'statements with predictive content.' This terminology is more sensitive to the fact that the statements about the empirical consequences of the periodic systems could differ in their degree of descriptiveness. Just as the statement "I will live in Europe next summer" gives a less descriptive statement about the future than "From June 30th onwards, I will live in

[56] Such corrections can be found from Mendeleev (1871c) and from Meyer's discussion of the tabular system of 1870 (1870, 357-58).
[57] See especially Newlands (1864b, 95), Meyer (1864, 137), Mendeleev (1869b, 31).
[58] This includes Mendeleev's discussion of eka-boron, eka-aluminium, eka-silicon (Mendeleev 1871c).
[59] This picks up Meyer's use of the atomic volume curve to identify errors in the experimental results of indium. I have distinguished this from correction of data, because Meyer emphasised that the corrections were just hypotheses and would be later justified by experimental data. Furthermore, Meyer's discussion of indium (see chapter 8) cannot be framed as correction of results, as Meyer did not frame his discussion in such a way.

France, on 27 Rue Carnon, in the town of Rouen," similarly "there will be an element filling this gap" is less descriptive, and thus weaker in its predictive content than the statement "this gap will be filled with an element that has atomic weight of 44 and that provides weakly basic oxides" (also Gordin 2004, 34 characterises some predictions as weak). Using the term "prediction" would effectively conceal such differences, whereas the language of predictive content draws attention to how content can vary. To reflect this difference in descriptiveness, the subcategories (i) and (ii) transition from weaker (less descriptive) predictive statements to stronger (more descriptive) ones when moving from left to right. In Popperian terms, the weaker statements stick their neck out less than the stronger predictive statements.

With this taxonomy, it becomes easier to articulate the relationship between completeness and predictions. In particular, I argue that Mendeleev's valuing of completeness during the development of the systems supported using the systems to determining the properties of little-known and unknown elements (i.e., correcting the properties of little-known elements, and describing the properties of unknown elements). Especially Mendeleev's inclusion of the row of oxides – which was one of the indicators of completeness – was crucial in guiding his predictions. As we will see, the row of oxides (together with other indicators of completeness) supported Mendeleev's process of predicting the properties of missing elements, which suggests that the valuing of completeness was instrumental in Mendeleev's application of the system to make predictions.

The structure of the discussion is as follows. I will start by zooming into one of the indicators of completeness – the schematic rows of oxides – and explain its significance for Mendeleev's overall project (section 7.2). After doing so, I argue that the row of oxides, together with other indicators of Mendeleev's valuing of completeness, supported his predictions of little-known elements indium and didymium, which turned out *not* to be an element (section 7.3). After discussing the little-known elements, I proceed to Mendeleev's descriptions of the properties of unknown elements. In section 7.4, I then explain how completeness supported predicting the properties of eka-boron (scandium).

While Mendeleev's predictions of eka-boron, eka-aluminium, and eka-silicon turned out to provide highly accurate descriptions of the properties of scandium, gallium, and germanium, Mendeleev did not only make successful predictions. Section 7.5 demonstrates, when Mendeleev predicted the properties of coronium and ether, he deprived himself of one of the most important parameters that guided his early predictions: the schematic row of oxides. This suggests that the valuing of completeness (especially with regards to chemical properties) supported making predictions.

7.2 Mendeleev's Valuing of Completeness and the Row of Oxides

In chapter 5, I identified six indicators of the valuing of completeness throughout Mendeleev's publications of 1869-1871 (see figure 7.3). One of the most striking visual indicators of completeness was Mendeleev's inclusion of schematic rows of oxides and hydrides to the system. As we saw, Mendeleev was the only chemist who included them in the layout of the system. Apart from giving us a visual clue of how Mendeleev incorporated many observations on chemical and physical properties of the elements to his system, the two rows also played a crucial role in applying the system to make predictions. In what follows, I will show how especially the schematic row of oxides was instrumental for making predictions of both the properties of unknown and little-known chemical elements. Examining its role gives us a sense of how completeness in accounting for chemical properties helped in making predictions.

(i) Inclusion of 63 elements in the system
(ii) Considering at least seventeen chemical and physical properties in relation to his system
(iii) Emphasis on the importance of completeness (*polnost'*) for a systematisation
(iv) Suggesting places for undiscovered elements
(v) Incorporating more information to the (visual) layout of the system
(vi) Evaluating the previous systematisation attempts in terms of completeness.

Figure 7.3 The indicators of completeness

As we saw in chapter 5, the main reason why Mendeleev introduced the schematic row of oxides was that it helped to show that the periodic system was a natural system rather than an artificial classification. In particular, the row of oxides drew attention to the gradual manner in which the elements on the horizontal lines of the system formed higher saline oxides.[60] To briefly recap the discussion of chapter 5, Mendeleev first realised this gradualness in forming oxides when he lined up the elements from 7-36 in the following two rows:

Li=7	Be=9.4	B=11	C=11	N=14	O=16	F=19
Na=23	Mg=24	Al=27.3	Si=28	P=31	S=32	Cl=35.5

As noted by Bensaude-Vincent (1986) these elements did not show any obvious resemblances. In particular, Mendeleev was struck by how the atomic weight corresponded to the manner in which the elements on the rows formed oxides. For example, the first row formed the following stable oxides:

Na=23	Mg=24	Al=27.3	Si=28	P=31	S=32	Cl=35.5
Na^2O	Mg^2O^2	Al^2O^3	Si^2O^4	P^2O^5	S^2O^6	Cl^2O^7
	or		or		or	
	MgO		SiO^2		SO^3	

Where this oxidisation can then be depicted in a more schematic manner as follows:

R^2O, R^2O^2, R^2O^3, R^2O^4, R^2O^5, R^2O^6, R^2O^7

Where each form of oxide would correspond to a vertical column of the system. As mentioned earlier, the row drew attention to the gradual transitioning across the horizontal rows, where this transitioning made Mendeleev convinced of the naturalness of his arrangement (1871c, 108). In what follows, I will explain how the row of oxides helped at

[60] Mendeleev also evoked the row of higher saline oxides as the first example of the periodicity of chemical properties in his briefer explanation of the periodic law in his essay on ether (Mendeleev 1904, 21-22).

making predictions about little-known and unknown elements. This gives a sense of how completeness supported predicting.

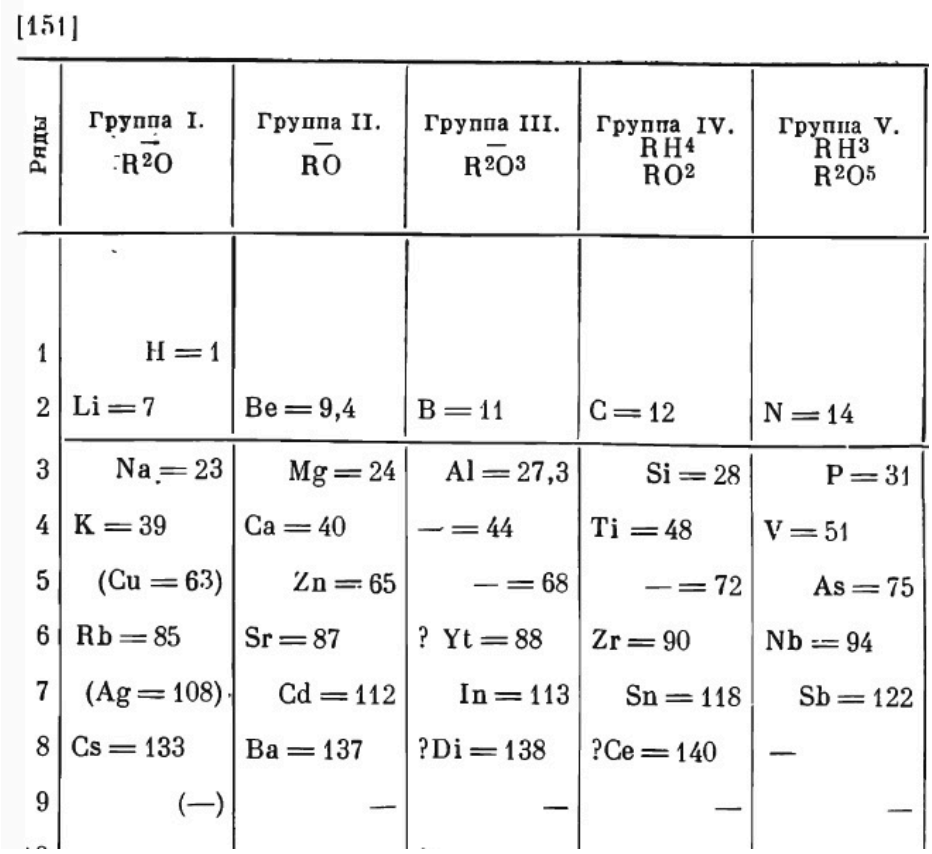
7.3 Predicting the Properties of Little-Known Elements

In this section, I demonstrate how the row of oxides helped Mendeleev to predict the properties of two little-known elements. To reflect the order in which Mendeleev presented his predictions, I will first present his predictions on indium and then didymium. After doing so, I will sum up how different indicators of the valuing of completeness came together to support Mendeleev's corrections of their properties.

Indium

Mendeleev's corrections to indium were presented as a discussion of a hypothetical element he called Ya. Mendeleev started by assuming that Ya was not very highly oxidizing and that its oxide would be quite weak (1871c, 133-34). With these assumptions, Mendeleev set out to show how the system could help to determine Ya's atomic weight and the formula for its oxide.

The first option was to place Ya in the first group on the system (see figure 7.4). The potential membership in this group implied that Ya's weight should be 38, which Mendeleev assumed on the grounds that the elements of group I form oxides of the form R^2O. However, the suitable spot in group I was already occupied by potassium (K=39). The lack of vacancies called for changing the form of Ya's oxide. Mendeleev suggested that the composition of its oxide should be changed from R^2O to RO, which altered Ya's atomic weight from 38 to 76 (38x2). However, the second group offered no place for an element with such weight, as zinc (Zn=65) and strontium (Sr=87) occupied those spaces.



[151]

Ряды	Группа I. $\dot{R}^2O$	Группа II. $\bar{R}O$	Группа III. $\bar{R}^2O^3$	Группа IV. RH^4 RO^2	Группа V. RH^3 R^2O^5
1	H = 1				
2	Li = 7	Be = 9,4	B = 11	C = 12	N = 14
3	Na = 23	Mg = 24	Al = 27,3	Si = 28	P = 31
4	K = 39	Ca = 40	— = 44	Ti = 48	V = 51
5	(Cu = 63)	Zn = 65	— = 68	— = 72	As = 75
6	Rb = 85	Sr = 87	? Yt = 88	Zr = 90	Nb = 94
7	(Ag = 108)	Cd = 112	In = 113	Sn = 118	Sb = 122
8	Cs = 133	Ba = 137	?Di = 138	?Ce = 140	—
9	(—)	—	—	—	—

Figure 7.4 Left side of Mendeleev's third system 1871 (1871c, 116).

Changing Ya's oxide from RO to R^2O^3 then altered its atomic weight from 76 to 114 (i.e., 38 x 3). This solution appeared to work well. The best spot for an element of this weight was still vacant, as there was a space between cadmium (Cd=112) and tin (Sn=118). Furthermore, the oxides of cadmium and tin suggested that the oxide of Ya should be weak, which was one of Mendeleev's original assumptions for Ya. Moreover, the missing element ought to form analogies with Al^2O^3 and Tl^2O^3 which seemed suitable.

The revelation of Mendeleev's treatment of Ya was that there was a rare element very much like Ya – indium. Indium's atomic weight had been assumed to be 75, but Mendeleev stated that it ought to be changed to 113, where its higher saline oxide would be of the form In^2O^3 and appear similar to aluminium (Al) and thallium (Tl) (members of group III) and cadmium (Cd) and tin (Sn) (on the 7th row) (1871c, 133-34). In particular, Mendeleev's discussion of Ya/indium reveals how he utilised the row of oxides to determine the atomic weights by guiding assumptions concerning the atomic weight of the element. However, the row of oxides was not the only guiding factor in Mendeleev's

descriptions of indium's properties. After he found a spot for indium on the system, Mendeleev provided a detailed account of its chemical properties on the basis of the properties of analogous elements on the system (1871c, 134-136).

Mendeleev first suggested that since cadmium (Cd) and tin (Sn) (both analogues of indium) were easy to regenerate in solutions of zinc, then indium, too, could be provided through similar means. Furthermore, since silver (Ag, in the 7th row, group I) was more refractory than cadmium, and antimony (Sb, in the 7th row, group V) was more refractory than tin, then judging by their example, Mendeleev predicted that indium must fuse more easily than cadmium (which fuses at 176°)" (1871c, 134). In addition, Mendeleev suspected indium would also have the silvery white colour of silver, cadmium, and tin.

To conclude Mendeleev's account of indium, I suggest that four of the indicators of completeness summarised in figure 7.3 were present in Mendeleev's predictions on indium's properties. Firstly, (i) we saw that Mendeleev's inclusion of 63 elements gave a greater number of comparisons for indium, which then allowed for drawing analogies from known elements to little-known ones. In a similar vein, (ii) Mendeleev's emphasis on bringing together as much data as possible helped to draw analogies between the better known elements (Ag, Cd, Sn) and the properties of indium. Thirdly, (iv) leaving out blank areas for the elements that were yet to be discovered allowed Mendeleev to identify a place for indium. Finally, (v) the inclusion of the row of oxides guided Mendeleev's assumptions of its weight and oxidisation. This suggests that the valuing of completeness was instrumental for Mendeleev's prediction of indium's properties.

Didymium

In 1842, Mosander discovered didymium (Di), but in contrast to indium, the element turned out to be a mixture of two other rare earth elements (Karpenko 1980, 91; Scerri 2007, 302).[61] In this section, I focus on Mendeleev's corrections of didymium's atomic weight and form of its oxide, which Mendeleev made during his more general discussion on the cerite metals. (As Mendeleev considered lanthanum in the same context, and I sought to

[61] Didymium turned out to be a mixture of praseodymium and neodymium (Karpenko 1980, 77; Scerri 2007, 302). For a further discussion on didymium, see Fontani, Costa and Orna (2015,122-124).

preserve the order in which Mendeleev presented his arguments, I have also included some of his descriptions of lanthanum here, too.) Again, I will show how especially the row of oxides guided Mendeleev's reasoning about the properties of didymium.

Mendeleev discussed didymium's atomic weight and oxidisation together with the other cerite metals. The close equivalent weights of lanthanum, didymium, and cerium (=45) together with their many shared properties rendered the determination of their atomic weights challenging (1871c, 141). Although many chemists regarded that they should be placed in the iron group (VIII), for Mendeleev, "a closer acquaintance with cerite metals calls for leaving such a line of thought behind" (1871c, 141). In order to demonstrate that cerium, lanthanum, and didymium should not be placed in the iron group, Mendeleev put forward two options for their placement. First, if their typical oxides were of the form RO, then their atomic weights would be approximately 92. Such weight and oxide would not allow to place the elements to group VIII (1871c, 141-2). Second, if their oxides were assigned the formula R^2O^3, then their atomic weights would be close to 138. Again, it would be difficult to place the elements of such weight into group VIII on 8th row, because the elements there had to have atomic weight of approximately (105+195)/2, or close to 150. Furthermore, the properties of the cerite metals suggested that they did not form analogies with metals of group VIII (1871c, 142).

While group VIII offered no place for the cerite metals, there were some other options on the table. After placing cerium in the system, Mendeleev considered lanthanum and didymium in conjunction. He stated that only didymium formed peroxide alongside the typical oxide, where it would dissolve in heat, and was unable to form salts (1871c, 145). For this reason, judging their atomic weight must be exclusively based on the typical salts of other oxides, the equivalents (=46) of which were quite close to one another (1871c, 145).

As the equivalents of didymium and lanthanum were very close, Mendeleev suggested three options for their placement on the system. First, they could be fitted to group III, between Ba=137 and Ce=140? on the 8th row (see figure 7.5). In this case, the equivalent of the metal would have to be 138/3=46, but Mendeleev noted that we should expect

mistakes in these estimations because of the impurities in the samples. In any case, when considering the analogical elements Cs, Ba, Ce, Mendeleev saw that the position on the eighth row on the group III would be suitable for an element that provided a clearly basic and not very volatile chloric salt, which seemed to correspond to the properties of didymium and lanthanum (1871c, 145-6).

The second potential spot for the elements was in the group IV, before Ta=182 on the 10th row, where the analogues of the element would be Ce=140? and Th=231. Mendeleev stated that the elements fitted there must exhibit atomic weight close to 180, oxide RO^2, and equivalent 45, which was close to that of lanthanum and didymium (1871c, 146). The third and final available position was in the group V in the 12th row, which would allow placing the elements between Th=231 and U=240. The elements placed there should have an oxide of the from R^2O^5, show atomic weight close to 235, and equivalent close to 49.

[151] ТАБЛИЦА 2

Ряды	Группа I. $\vec{R^2O}$	Группа II. $\bar{R}O$	Группа III. $\bar{R^2O^3}$	Группа IV. RH^4 RO^2	Группа V. RH^3 R^2O^5	Группа VI. RH^2 RO^3	Группа VII. RH R^2O^7	Группа VIII. $\bar{R}O^4$
1	H = 1							
2	Li = 7	Be = 9,4	B = 11	C = 12	N = 14	O = 16	F = 19	
3	Na = 23	Mg = 24	Al = 27,3	Si = 28	P = 31	S = 32	Cl = 35,5	
4	K = 39	Ca = 40	— = 44	Ti = 48	V = 51	Cr = 52	Mn = 55	Fe = 56, Co = 59,
5	(Cu = 63)	Zn = 65	— = 68	— = 72	As = 75	Se = 78	Br = 80	Ni = 59, Cu = 63.
6	Rb = 85	Sr = 87	? Yt = 88	Zr = 90	Nb = 94	Mo = 96	— = 100	Ru = 104, Rh = 104,
7	(Ag = 108)	Cd = 112	In = 113	Sn = 118	Sb = 122	⊙Te = 125	J = 127	Pd = 106, Ag = 108.
8	Cs = 133	Ba = 137	?Di = 138	?Ce = 140	—	—	—	— — — —
9	(—)	—	—	—	—	—	—	
10	—	—	?Er = 178	?La = 180	Ta = 182	W = 184	—	Os = 195, Ir = 197,
11	(Au = 199)	Hg = 200	Tl = 204	Pb = 207	Bi = 208	—	—	Pt = 198, Au = 199
12	—	—	—	Th = 231	—	U = 240	—	— — — —

Figure 7.5 The third system of 1871 (1871c, 116).

Mendeleev stated that the third option was far from optimal. This was because the equivalent needed for this position was higher than that of didymium and lanthanum. Furthermore, the oxide had to provide a weak base, or a less energetic one than ThO^2. However, the oxides of lanthanum and didymium were clearly basic. Finally, both the oxide and the metal of the group V had to be quite heavy, where the analogues of oxides of didymium and lanthanum suggested specific weight no higher than 6.5 (1871c, 146).

Mendeleev concluded his discussion by stating that lanthanum and didymium should be placed to the first two available vacancies, where the oxide of one of the elements would have the form R^2O^3 and the other RO^2. One of them should then appear similar to the lower oxide of cerium and the other to cerium's higher oxide (1871c, 146). However, as the properties of didymium and lanthanum appeared similar, the assumption of the difference of their oxidisation required some further motivation. To support the argument that the two elements provided oxides of different form, Mendeleev raised Marignac's data. In particular, Marignac's observations suggested that the salts of lanthanum and didymium were not isomorphous, and the composition of sulphuric-didymium salts and sulphuric-lanthanum salts were different. More specifically, Marignac's investigations suggested that the former was of the composition $3DiSo^48H^2O$ and the latter $LaSO^43H^2O$. This was so if one assumed that the formula for their typical oxide was of the form RO. These differences in composition suggested to Mendeleev that formula R^2O^3 was more appropriate for the oxide of didymium than for lanthanum. Endorsing this assumption would change the formula for sulphuric-didymium salts to $Di^2(SO^4)^38H^2O$ and the sulphuric lanthanum salts to $La(SO^4)^26H^2O$ (1871c, 146). Mendeleev concluded that it was best to think that didymium's atomic weight was 138 and that it should be fitted to group III, had a higher saline oxide of the form Di^2O^3, whereas lanthanum has the atomic weight 180, should be fitted to group IV, and provided a higher saline oxide of the form LaO^2 (1871c, 146).

The above discussion indicates how the row of oxides guided Mendeleev's assumptions concerning the atomic weight and the chemical properties of didymium. We saw that the row of oxides ruled out the option that the cerite triplet should be placed in group VIII.

More specifically to didymium, Mendeleev identified three vacancies that it could occupy in the system. The row of oxides allowed him to narrow down the options, as we saw in his discussion of the third option (group V placement). The vacancy was to be filled by a weakly basic element, but the observed properties of didymium and lanthanum suggested that they were more strongly basic. For this reason, it was more appropriate to place didymium in the more basic III group and lanthanum to IV (where Marignac's data helped to show that their oxides can plausibly be thought to have different forms). This suggests that taking into account the gradual transitioning from greater basicity to acidity during the development of the systematisation helped to shepherd didymium and lanthanum to the most appropriate free positions on the system.

To sum, the following three indicators of completeness guided Mendeleev's discussion of didymium (and lanthanum). Firstly (v) the schematic row of oxides guided Mendeleev's assumptions on their atomic weights and the forms of their higher saline oxides. Secondly, Mendeleev's consideration of (ii) basicity and volatility helped show that the first option was a suitable vacancy for didymium/lanthanum. We then saw that the properties of oxides (acidity and basicity) helped Mendeleev to rule out the third option for the placement of didymium/lanthanum. Finally, suggesting places for undiscovered elements (iv) gave Mendeleev several options for placing didymium/lanthanum.

As noted in the opening section, didymium is usually brought up as an example of Mendeleev's 1871 predictions going astray (Karpenko 1980, 77). However, it would be important to take into account those predictions which we retrospectively have deemed as "unsuccessful" (for Mendeleev's unsuccessful predictions, see especially Scerri 2007, Ch.5). This is because Mendeleev's unrealised predictions show the limits to articulating the relationship between the valuing of completeness and using the system to make predictions. In particular, didymium suggests that it would be inappropriate to argue that completeness was a necessary or a sufficient condition for *accurate* predictions. Rather, its example demonstrates that the indicators of completeness only supported *making* detailed predictions.

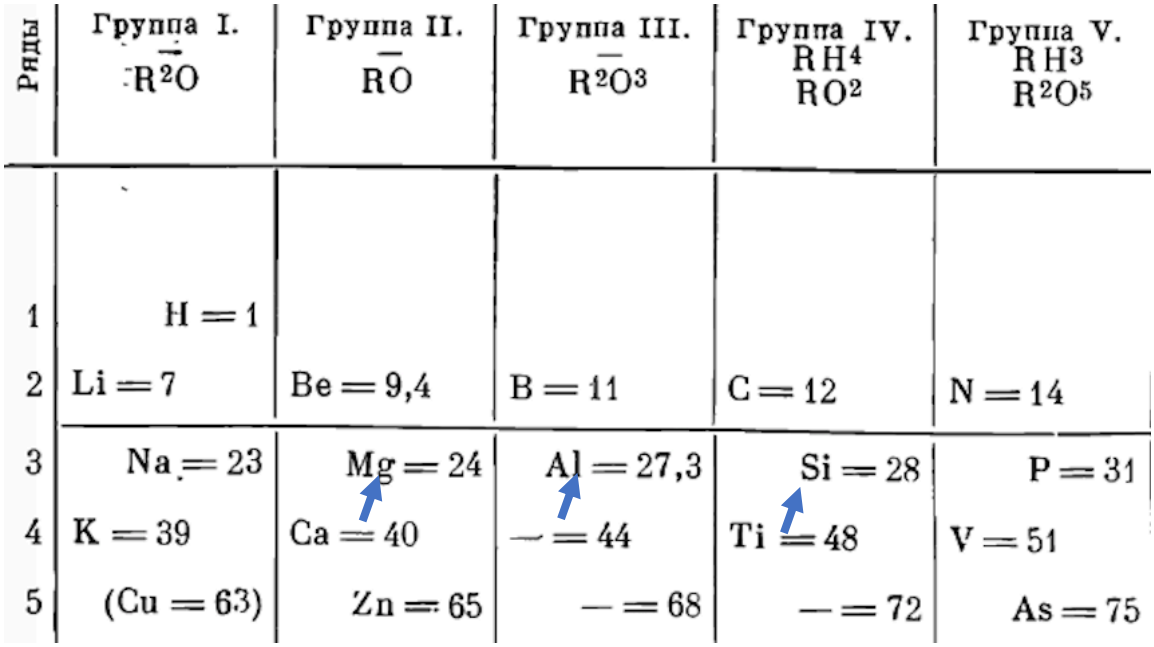
7.4 Unknown Elements of 1871: Eka-boron

As noted earlier, indium was an already discovered element, albeit it was far less well known and studied than some of the other elements included in the system. Eka-boron (scandium) gives us a different example to indium and didymium. Eka-boron was not a little-known element, but an unknown one that Mendeleev expected to enter the periodic system. Both with indium and didymium, we saw that the periodic system was used to test different positions for the elements. Interestingly, with eka-boron, Mendeleev applied the periodic system in a slightly different manner; he first assigned it its position, and only then used the system to visualise analogies and similarities. However, similarly to indium and didymium, completeness in accounting for chemical properties (especially the row of oxides) helped in describing the properties of eka-boron. As Mendeleev's predictions are numerous, here I focus only on those concerning eka-boron's oxide.[62]

Figure 7.6 demonstrates that Mendeleev suspected eka-boron would find its place on the fourth row, where it would be preceded by potassium (K=39), calcium (Ca=40) and followed by titanium (Ti=48). The placement suggested that its atomic weight had to be close to 44 and its oxide of the form R^2O^3 (1871c, 151). Mendeleev suspected that in all its "relations" (i.e., its chemical properties) eka-boron's oxide should be the average between the properties of its two neighbours: calcium oxide (CaO) and titania (TiO^2) (1871c, 151). As we can see from the table, this corresponds to eka-boron's position between the two.

Mendeleev then predicted that Eb^2O^3 must have a similar relationship to the oxide of aluminium (alumina) above it as can be observed on eka-boron's left-side (CaO, MgO) and its right-side (TiO^2, SiO^2). In other words, Mendeleev expected that the relationship between Eb^2O^3 and alumina had to be similar as the relationships between calcium oxide (CaO), its own upstairs neighbour magnesia (MgO), and the relationship between titania (TiO^2) and silica above (SiO^2).

[62] As noted in the introduction to this chapter, the description of its properties turned out to match the properties of scandium remarkably well. For a comparison between the predictions and scandium's properties, see Scerri (2007, 138).

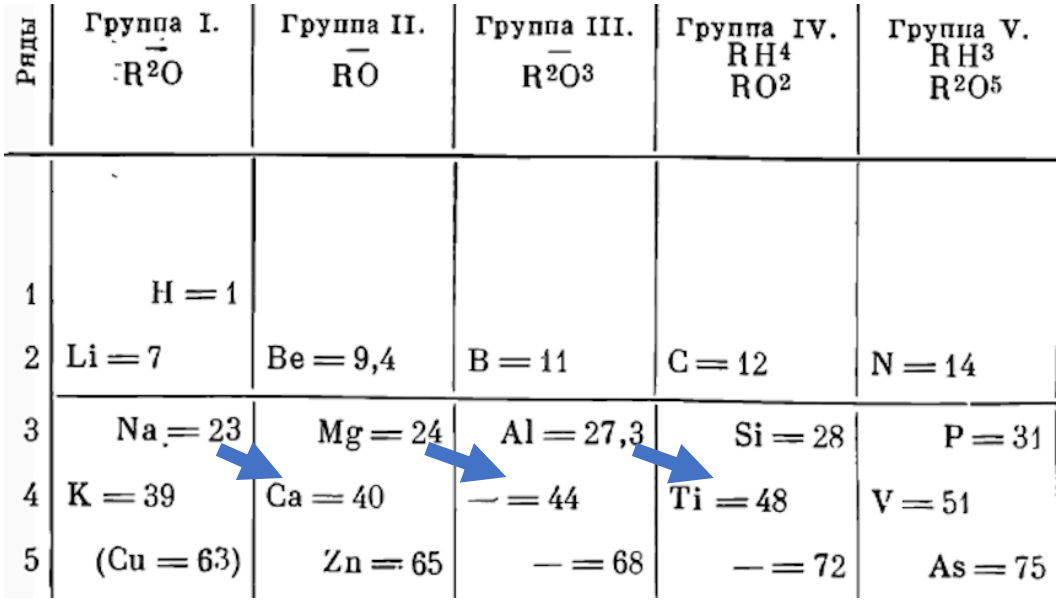


Ряды	Группа I. $\overrightarrow{R^2O}$	Группа II. $\overline{RO}$	Группа III. $\overline{R^2O^3}$	Группа IV. RH^4 RO^2	Группа V. RH^3 R^2O^5
1	H = 1				
2	Li = 7	Be = 9,4	B = 11	C = 12	N = 14
3	Na = 23	Mg = 24	Al = 27,3	Si = 28	P = 31
4	K = 39	Ca = 40	— = 44	Ti = 48	V = 51
5	(Cu = 63)	Zn = 65	— = 68	— = 72	As = 75

Figure 7.6 Extract from the third system of 1871 (1871c, 116)

Mendeleev's next prediction concerned the more specific chemical properties of Eb^2O^3. In particular, he suspected that Eb^2O^3 ought to be more basic than alumina. Again, Mendeleev made this assumption on the basis of the behavior of eka-boron's neighbours. On the left of eka-boron, calcium oxide (CaO) appeared more basic than magnesia (MgO), and to the right of eka-boron, titania (TiO^2) appeared more basic than silica (SiO^2). Thus, Mendeleev expected that the eka-boron-alumina relationship formed no exception to the trend. In other words, as the ground-floor neighbours of eka-boron were more basic than the first-floor inhabitants, and eka-boron was the average between its neighbours, then one should expect it to be more basic than alumina (1871c, 151).

After suggesting that Eb^2O^3 ought to be more basic than alumina, Mendeleev suspected that magnesia should be more basic than Eb^2O^3. As calcium oxide on eka-boron's left side is less basic than sodium oxide (Na), and as titania TiO^2 less basic than alumina (Al^2O^3), we ought to see a similar relationship between eka-boron's oxide and magnesia (see figure 7.7).



Ряды	Группа I. $\vec{R^2O}$	Группа II. $\bar{RO}$	Группа III. $\bar{R^2O^3}$	Группа IV. RH^4 RO^2	Группа V. RH^3 R^2O^5
1	H = 1				
2	Li = 7	Be = 9,4	B = 11	C = 12	N = 14
3	Na = 23	Mg = 24	Al = 27,3	Si = 28	P = 31
4	K = 39	Ca = 40	— = 44	Ti = 48	V = 51
5	(Cu = 63)	Zn = 65	— = 68	— = 72	As = 75

Figure 7.7 Extract from the third system of 1871 (1871c, 116)

The suggestions on the greater basicity conclude Mendeleev's predictions on eka-boron's oxide. Similarly to indium and didymium, the valuing of completeness – especially the indicators (i), (ii), (iii), (iv), and (v)– supported these predictions on eka-boron's oxide. Firstly, the inclusion of (i) all of the substances that were suspected to be elements allowed Mendeleev to demonstrate visual analogies more thoroughly. Secondly, (ii and iii) Mendeleev's emphasis on bringing together as much data as possible allowed him to draw analogical inferences between better-known elements for describing the chemical properties of eka-boron. Thirdly, the inclusion of gaps (iv) allowed him to find a place for undiscovered eka-elements in the periodic system. Finally, (v) Mendeleev's inclusion of the row of oxides grounded the predictions concerning properties Eb^2O^3. Thus, completeness supported predicting properties of eka-boron.

So far, I have argued that the inclusion of the row of oxides – amongst other indicators of completeness – supported making predictions in two ways: by guiding atomic weight calculations and by highlighting a gradual transition on how the elements of each group formed higher saline oxides. This example suggests that completeness supported making predictions. However, Mendeleev is also known for making a number of unsuccessful predictions on the properties of undiscovered elements (Scerri 2007, 140-145; Bensaude-

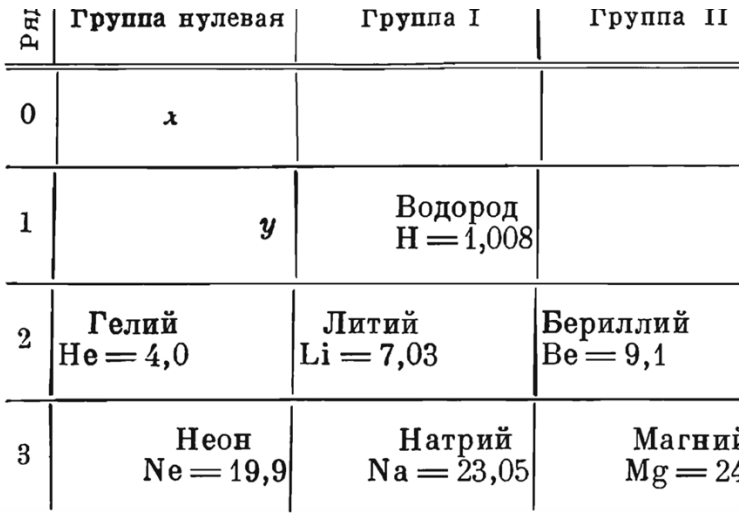
Vincent 1982, 183). In what follows, I will explore the relationship between the valuing of completeness and Mendeleev's predictions of coronium and ether.

7.5 Mendeleev's Predictions on Ether and Coronium

When we contrast Mendeleev's early predictions of 1871 with his later predictions of 1904, it becomes apparent that his subsequent predictions of the properties of coronium and ether did not make use of the completeness of the periodic system in the same manner as with eka-boron or indium. This is because Mendeleev assumed that ether (element x) and coronium (element y) were extremely light and inert, which made drawing analogies difficult.[63] More specifically, the lightness of x and y placed them above all the other elements in the system, so they were not embedded within its framework in the same way as indium, didymium/lanthanum, or ekaboron had been (see figure 7.8). Thus, their lightness made it difficult to draw analogies between the known elements and unknown ones. Furthermore, especially the inability of x and y to combine chemically had the consequence that Mendeleev could not utilise one of his most essential guides in making predictions: the knowledge of different chemical properties. With fewer chemical qualities to work with, the predictions of coronium and ether were not *as* rooted in analogical reasoning as the early predictions of 1871. This suggests that completeness in accounting for *observed* chemical and physical properties supported determining the properties of unknown elements.

Both the predictions of coronium and ether featured in Mendeleev's short book published in English in 1904 (Mendeleev 1904, 51). In this book, written in 1902, Mendeleev stated that many explanations of natural phenomena required assuming the existence of the ether (1904, 1). Mendeleev saw that the ether had to behave like matter and have a weight, but also be radically different from vapours and gases as it had to be an "all-pervading ubiquitous substance" (1904, 4). Furthermore, Mendeleev expected it to be a substance with a "chemical nature" (1904, 7).

[63] For the role of ether in science more generally, see Navarro (2012, Ch.3; 2018). For "lost elements" which were initially suspected to be the chemical elements, but turned out to be false entries, see Fontani, Costa, and Orna (2015; for their discussion of Mendeleev's ether and coronium, see pp. 419-422).



Ряд	Группа нулевая	Группа I	Группа II
0	*x*		
1	*y*	Водород H = 1,008	
2	Гелий He = 4,0	Литий Li = 7,03	Бериллий Be = 9,1
3	Неон Ne = 19,9	Натрий Na = 23,05	Магний Mg = 24

Figure 7.8 Extract from the Russian language version of the table of 1904 (Kedrov 1958a, 422). For the English version, see Mendeleev (1904, 26). Note that in contrast to his early systems, here Mendeleev expressed atomic weights more precisely.

Explanations as to why Mendeleev saw that such an un-substance-like substance should have a distinctly chemical nature came from the investigations of Lord Rayleigh (John William Strutt) and William Ramsay. The British chemists had discovered argon, "the most inactive element," where their discovery was soon followed by the identification of helium, neon, krypton, and xenon (1904, 13). For Mendeleev,

> None of these five new gases have yet given any definite compounds, although they clearly evince the faculty of solution, *i.e.* forming indefinite, easily dissociated compounds. Thus we have now every right to say that the ether is unable to form any stable compounds with other chemical atoms, although it permeates all substances. (1904, 14).

On these grounds, Mendeleev saw it safe to assume that ether would be a gas, but incapable of stable chemical combination (despite his initial emphasis that ether was also very different from gases and vapours) (1904, 14). In particular, Mendeleev expected ether to be "an elastic fluid having no cohesion between its parts," and similarly to all other simple bodies or elements, it had to have a weight (1904, 15).

After suggesting that the ether was a gas incapable of chemical combination, Mendeleev set to predict its atomic weight. Measuring its mass was very difficult because it was impossible to experimentally determine the weight of such a light element. Despite this, Mendeleev saw that he could predict its atomic weight. Before making such predictions, however, Mendeleev briefly contrasted his task with his 1871 predictions of gallium, scandium, and germanium. He stated that

> I made these predictions by following what is known in mathematics as a method of interpolation, that is, by finding intermediate points by means of two extreme points whose relative position is known. The fact of my predictions having proved true confirmed the periodic system of the elements, which may now be considered as an absolute law. So long as the law remained unconfirmed, it was not possible to extrapolate (*i.e.* to determine points beyond the limits of the known) by its means, but now such a method may be followed, and I have ventured to do so in the following remarks on the ether, as an element lighter than hydrogen. (1904, 16-17).

This demonstrates the following key difference between Mendeleev's early and later predictions: the predictions of 1871 were informed by his knowledge of the properties of the known elements, whereas the predictions of the properties of ether and coronium drew from the analogies between known and unknown elements to a much lesser extent. Interestingly, Mendeleev acknowledged that he could not predict the properties of ether with quite as much confidence as he had done thirty-three years earlier. Nevertheless, he saw it as an urgent task (1904, 18).[64]

After discussing the placement of the inert gases, Mendeleev predicted the properties of two unknown elements: x and y. Mendeleev first considered the element he named y (coronium), which he fitted in the first series (horizontal line) of group zero (vertical line). He suspected coronium would show properties similar to argon (1904, 27). With these

[64] While Mendeleev's age is evoked as an explanation for taking the ether seriously, Bensaude-Vincent has demonstrated that such arguments are misleading – evoking of the ether was not unusual in the nineteenth century. For example, Fresnel and Maxwell brought up ether, where Mendeleev discussed Lord Kelvin's treatment of the ether (Bensaude-Vincent 1982, 184; Mendeleev 1904, 2).

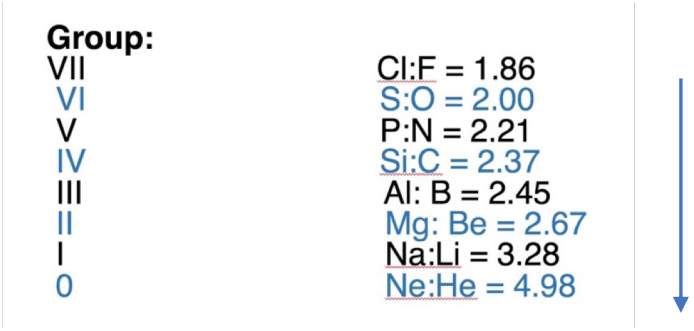


Figure 7.9 Atomic weight ratios between the elements (Mendeleev, 1904, 27).

assumptions, Mendeleev determined coronium's atomic weight, which he predicted on the basis of the atomic weight ratios of pairs of elements on the same group in the neighbouring series, displayed above on figure 7.9.

In particular, Mendeleev drew attention to how the ratio "in the given series distinctly and progressively increases in passing from the higher to the lower groups" (1904, 28). As the ratio increased when transitioning from higher groups to the lower ones, Mendeleev expected that the ratio between helium (He) and coronium (He:y) to be "considerably greater" than Li: H=6.97 – at least 10, or even greater (1904, 28). As He=4.0, Mendeleev saw that the weight of coronium ought to be 4.0/10.

While Mendeleev did not consider observed qualities beyond atomic weight ratios in his predictions of coronium's atomic weight, he evoked observations of a potential unknown element in the spectra of volcanic ashes (1904, 28). These observations seemed to point toward an element like coronium, and gave enough grounds for Mendeleev to predict the density of coronium with Avogadro-Gerhardt's law. If 0.4 was taken as the greatest possible atomic weight of coronium, its density (in relation to hydrogen) should be less than 0.2 (unfortunately Mendeleev did not elaborate why). For Mendeleev, these assumptions on the weight and density of coronium would make it light enough to escape the gravity of earth, but sufficiently heavy and dense to be drawn towards the sun (1904, 30).

These remarks conclude Mendeleev's predictions of the properties of coronium, which Mendeleev regarded as necessary for determining the properties of "the lightest and

therefore the swiftest element, x," the ether. Similarly to coronium, ether was inert and light. More specifically, Mendeleev thought that ether must be the most mobile, the lightest (both in density and weight), the least prone to combine with other elements, and an "all-permeating and penetrating substance" (1904, 31). Although the atomic weight ratios of xenon, argon, chromium, and helium (from ether's group) implied that x/ether's atomic weight should be about x=0.17, Mendeleev thought that it should be much lighter than that. An all-permeating ether had to have a weight so low that it would not be attracted by the weight of earth, the sun, or the stars (1904, 34). These requirements called for placing it above hydrogen. To make such an accommodation, Mendeleev had to make quite a drastic addition to the system, and include a whole new zero series on the horizontal row for placing ether.

As noted earlier, the lightness of ether had the inconvenient consequence that its atomic weight could not be determined through laboratory means. However, Mendeleev saw that there were other ways to predict its weight. Mendeleev argued that informed guesses about ether's weight were possible if its velocity and temperature were known. Here, the ideal gas law could be used to estimate its molecular speed or velocity. If ether's weight is assumed to be x and its density x/2, then the velocity of the molecule could be calculated with the following equation:

$$v = 1843\sqrt{\frac{2(1+at)}{x}}$$

Where 1843 refers to the already determined velocity of hydrogen at 0 degrees (1904, 35). As to t (temperature), Mendeleev thought it should correspond to the temperature of space, which, unfortunately, could not be exactly calculated. Despite this, Mendeleev thought it reasonable to assume that the temperature of space was approximately -80° (1904, 36). As one of Mendeleev's original assumptions was that ether is light enough to escape the gravity of earth and stars, Mendeleev could rely on the estimations on the mass of earth and some stars to calculate the velocity of ether. After putting forward such calculations, he suggested that velocity of ether ought to be at least 2,240km/sec but less

than 300 000 km/sec.[65] This would suggest that the atomic weight of ether must be between 0.000,000,96 and 0.000,000,000,053 if hydrogen is taken as a unit (1904, 41-42). Mendeleev concluded that "*the particles and atoms of the lightest element x capable of moving freely everywhere throughout the universe have an atomic weight nearly one millionth that of hydrogen, and travel with a velocity of about 2,250 kilomètres per second*" (1904. 43, emphasis original).

This concludes Mendeleev's reflection on ether. What do coronium and ether tell us about the relationship between the valuing of completeness and using the system to make predictions? For starters, it is clear that Mendeleev did not use the system to demonstrate analogies between the elements as much as he did with the early predictions. With coronium and ether, Mendeleev only stated that the inert gases justified the assumption that they were gaseous and incapable of chemical combination. The only strong analogy that Mendeleev drew on the basis of other elements (the inertness) ruled out the possibility that they had substantial chemical properties (as knowledge on chemical properties rested on knowledge on chemical reactions). Furthermore, when Mendeleev utilised data in determining the properties of coronium, the data was largely quantitative. Mendeleev's only qualitative remark on their physical state concerned their gaseous nature.

In sum, the lack of analogical inferences between empirical observations and unknown elements suggests that Mendeleev's assumption of inertness limited drawing analogies in the way he had done with eka-elements. The assumed inertness of coronium and ether seemed to deprive Mendeleev of two of the principles that guided his early predictions: empirically determined atomic weights and chemical combinations.[66] This suggests that completeness in accounting for observed phenomena was instrumental for predictions that were done by drawing analogies between the known elements and unknown ones.

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[65] Mendeleev presented this value in meters rather than kilometres, but for the sake of drawing comparisons, I converted his units to kilometres.

[66] Apart from limiting the number of chemical data to guide his predictions, Bensaude-Vincent (1982) identifies a number of other reasons for the failure of these later predictions. For example, Mendeleev did not maintain his distinction between elements and simple substances. In predicting the properties of ether, the simple substances and elements became interchangeable.

7.6 Conclusion.

Earlier in this chapter, I put forward a taxonomy that proposes some finer distinctions between the predictive statements found in the publications of Newlands, Meyer, and Mendeleev publications. The aim of this chapter was to show that Mendeleev's emphasis on completeness during the development of the system supported making predictive statements. More specifically, I showed how several different indicators of completeness came together in both his corrections of the atomic weights of little-known elements and his predictions of the properties of entirely unknown elements. We saw that completeness supported especially his descriptions of the properties of indium, didymium, and eka-boron. This argument can also be extended to eka-aluminium and ekasilicon. However, we saw that Mendeleev's later predictions of 1904 came with a corresponding incompleteness of chemical details. This had more to do with Mendeleev's assumption about the inertness of ether and coronium than the disvaluing of completeness.

Chapter 8. Carefulness, Completeness, and Error Identification

8.1 Introduction

In chapter 7, I argued that the valuing of completeness supported Mendeleev's use of the periodic system for making predictions. The task of this chapter is analogous. I will show that both Meyer's valuing of qualitative completeness and carefulness supported using his system to identify errors in experimental results. In particular, the valuing of carefulness in selecting the observations rendered Meyer's graph useful for identifying errors in experimental results.[67] With qualitative completeness, we will see that the qualities that Meyer considered during systematisation helped him to rule out the least likely atomic weights (and volumes) for indium.

The structure of this chapter is as follows. In section 8.2, I explain how Meyer used his atomic volume graph to identify errors in the data on indium and caesium. In section 8.3, I then show how completeness and carefulness related to Meyer's use of the curve. As we will see, especially the valuing of carefulness influenced Meyer's use of the system for identifying errors. Section 8.4 will then suggest that valuing of carefulness was crucial for Meyer's broader project of demonstrating the helpfulness of theories in chemistry.

8.2 Identification of Errors

There are many settings in which Meyer identified errors in the atomic weight determinations (see especially Meyer & Seubert, 1883). However, in this section, I will focus specifically on those cases where Meyer employed his periodic systems to identify errors. I will especially concentrate on Meyer's article of 1870 and the 1876 edition of his textbook. As we will see, in both settings, the principle of applying the systems was similar: if the empirical results differed from the regularity indicated by the system, then there were good grounds to suspect that the empirical results needed reassessment.

[67] Earlier, I defined carefulness as *involving signalling differences in the quality of observations of the phenomena on the problem area, and prioritizing observations of higher quality over those that are lacking.*

In chapter 4, we saw that Meyer introduced a graph to track the periodic dependency between atomic volumes and atomic weights in his paper of 1870. Although Meyer implied that the curve could be used as a diagnostic tool to identify errors in atomic weight determinations more generally, he illustrated this use only in detail with indium. In particular, Meyer suggested that indium's atomic weight needed some reassessment (1870, 363). Meyer saw that there were three plausible options for its atomic weight: (i) 37.8, (ii) 75.6 (2 x 37.8), or (iii) 113.4 (3 x 37.8). The first option of 37.8 involved accepting that indium's volume was 5.1. Meyer argued that this could not be its volume, as indium would then "lie entirely outside the curve" (1870, 363). Figure 8.1 effectively illustrates this. The second option was to take 75.6 as the atomic weight of indium. This option would place it between arsenic (As) and selenium (Se). However, Meyer discarded the second option, because a ductile electropositive metal such as indium would not fit well between the metalloid arsenic and the non-metallic selenium (1870, 363). Furthermore, the atomic weight of 75.6 would make its volume 10.7, which did not fit the curve (see figure 8.2). The third option – the weight of 113.4 – worked much better. This weight allowed placing indium between cadmium (Cd) and tin (Sn) on the system, where its atomic volume would be 15.3, which fit "quite well on the curve" (1870, 363). If we examine the curve, we can find indium in this position (see figure 8.3).

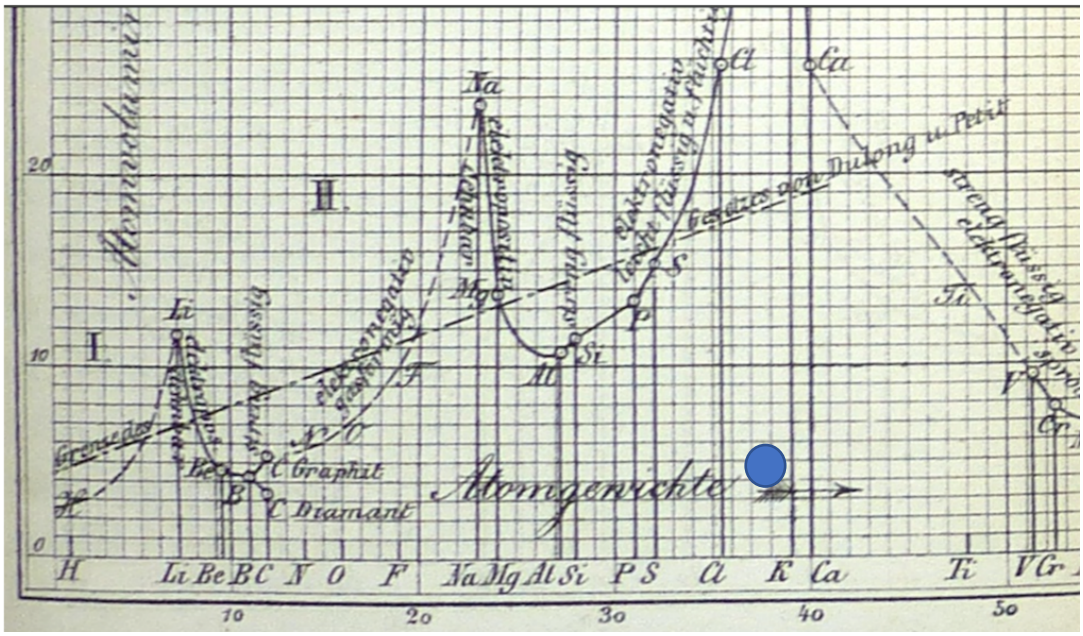

Figure 8.1 Extract from Meyer's graph of 1870, with the first option for indium marked with a dot.

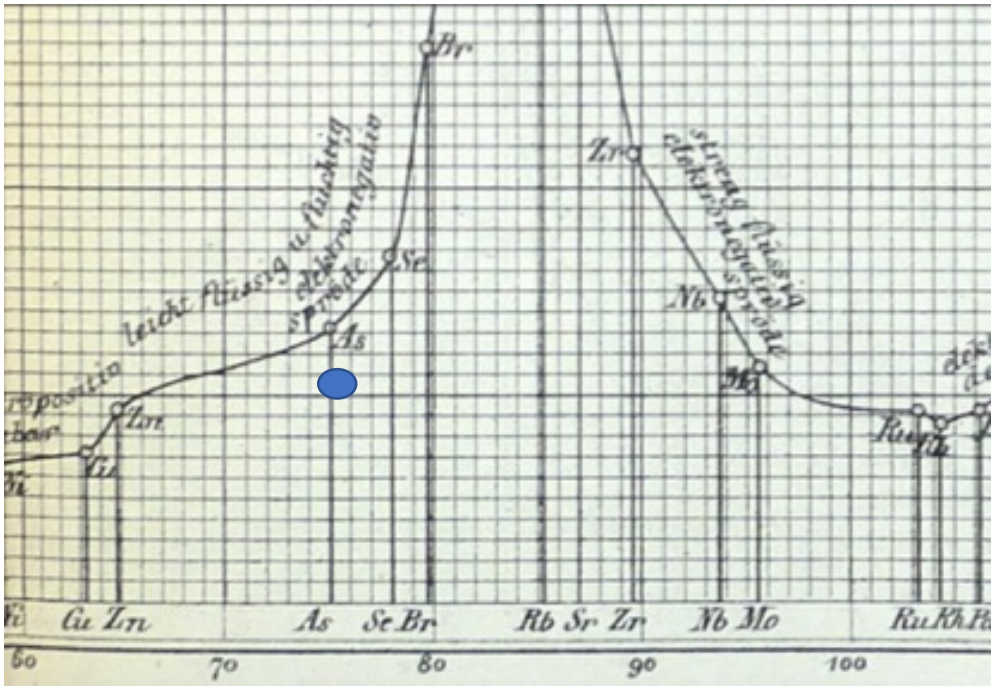

Figure 8.2 Extract from Meyer's graph of 1870, with the second option for indium marked with a dot.

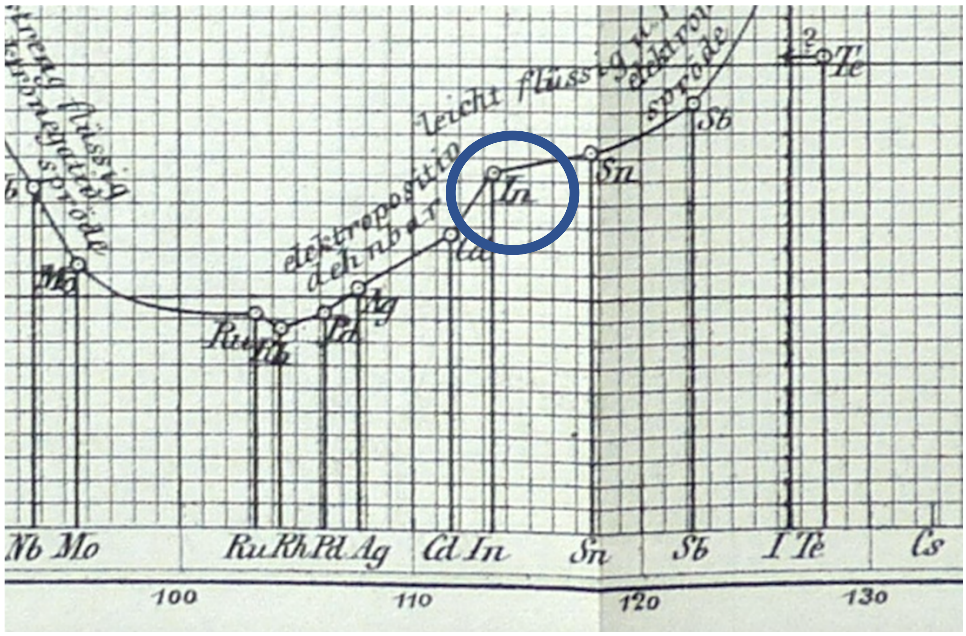

Figure 8.3 Extract from Meyer's graph of 1870, with the third option for indium marked with a circle.

A similar example of Meyer's use of the graph can be found from his textbooks. Although Meyer stressed the uncertainty of the information on how chemical properties depended on atomic weights, he maintained that one could utilise such information for "eliminating

errors and broadening our knowledge" (1876, 326).[68] To illustrate such a use, Meyer brought attention to caesium's atomic weight determinations. When Bunsen proposed that caesium's atomic weight should be 123.4, Meyer saw that the atomic weight "interfered with the regularity of the differences between the atomic weights of alkali metals" (1876, 326). The regularity amongst the alkali metals could be expressed as follows:

Li = 7.01	Na = 22.99	K = 39.04	Rb = 85.2	(Cs = 123.4).
Difference: 15.98	16.05	46.16	38.2 .	

In the above row, we can observe how the difference increases from Li-Rb, but drops after caesium. This sudden change gave Meyer the reason to doubt Bunsen's determination of caesium's atomic weight. Bunsen's atomic weight determination was not the only option on the market; in 1863, Johnson and Allen suggested that the atomic weight of caesium was 132.7. In the light of the regularity amongst alkali metals, Meyer argued that the result of Johnson and Allen was the preferable option to that of Bunsen's, as it did not deviate from the regularity amongst alkali metals. (Later, Bunsen's determinations confirmed the result of Johnson and Allen.) Thus, Meyer's discussion gives us an example of how Meyer evaluated competing atomic weight determinations on the basis of how well they matched his theoretical estimates.

Both Meyer's discussion of indium and caesium raise the following question: how is Meyer's use of the atomic volume curve different from Mendeleev's determination of the properties of little-known elements? This question is especially pressing with Meyer's treatment of indium, as Mendeleev's discussion appears highly similar to it. Recall that in chapter 7, I introduced the following taxonomy of chemists' statements with predictive content:

[68] "...doch hat sie sich schon in manchen Fällen als ein nützliches Hülfsmittel zur Beseitigung von Irrthümern und zur Erweiterung unserer Kenntnisse erwiesen" (Meyer 1876, 326).

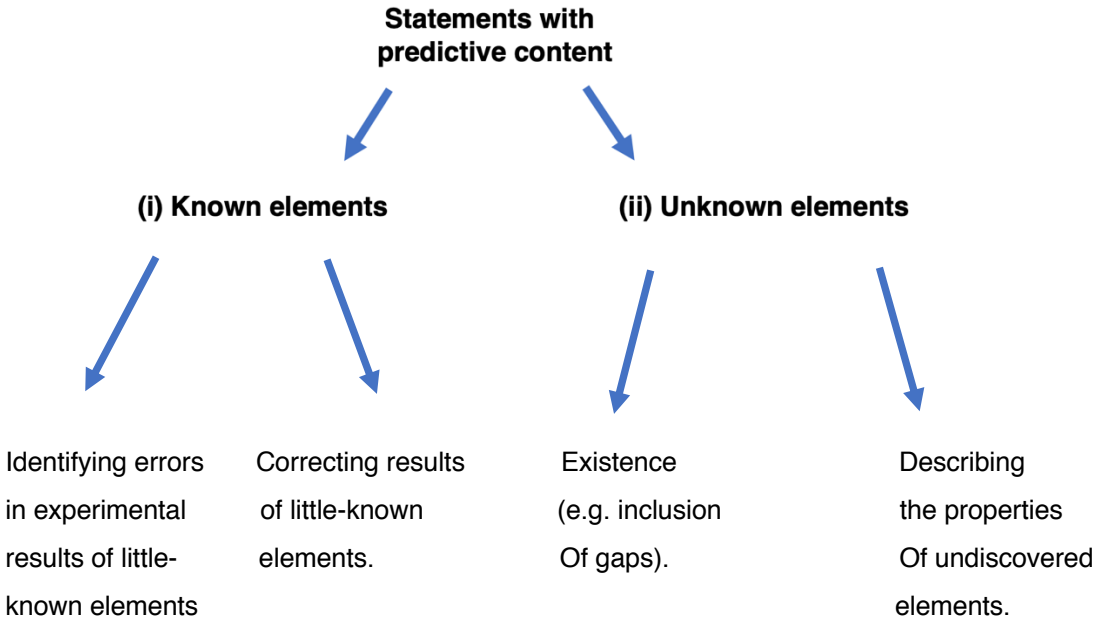

Figure 8.4 The taxonomy of predictive statements

As can be seen from figure 8.4, I have distinguished between statements that *identify* errors in experimental results and statements that *correct* results of little-known elements. Recall that the subcategories (i) and (ii) transition from weaker (less descriptive) predictive statements to stronger (more descriptive) ones when moving from left to right. If we conceptualise Meyer's use of the system with the help of the taxonomy, it suggests that Meyer's error identifications were weaker in their predictive content than the corrections of Mendeleev. After all, there is a difference in degree when we identify that there may be errors in something versus when we state how those errors should be corrected. Although Meyer suggested options for indium's atomic weight, his suggestions should not be taken as straightforward corrections. This is because he stated that it would be premature to change atomic weights on the basis of the atomic volume curve. Instead, for Meyer, the curve just suggested areas of uncertainty, and gave clues as to which atomic weights needed redetermination through experimental means.[69]

[69] "Es würde voreilig sein, auf so unsichere Anhaltspunkte hin eine Aenderung der bischer angenommen Atomgewichte vorzunehmen. Ueberhaupt darf man für jetzt Argumenten der angegebenen Art kein allzugrofses Gewicht beilegen, noch von ihnen eine so sichere Entscheidung erwarten, wie sie die Bestimmung der specifischen Wärme oder der Dampfdichte zu geben vermag" (Meyer 1870, 364).

There are also additional reasons that warrant treating Meyer's and Mendeleev's applications as different despite their apparent similarity. Meyer referred to his application of the "knowledge of the dependence of the properties and behaviour of the elements on the size of their atomic weight" – as expressed by the atomic volume curve – as error estimation or error elimination (*Beseitigung von Irrthümern*).[70] Furthermore, even when Meyer was at his most permissive about predicting the properties of the elements, he highlighted that doing so was permitted solely for testing the quality of the theories that gave rise to the predictions (1872, 344). In contrast, Mendeleev's treatment of indium was supposed to demonstrate the usefulness of the law of periodicity in predicting the properties of little-known elements (1871c, 131-150). The difference is not just cosmetic. For Mendeleev, predictions helped to demonstrate the naturalness of the system, and to show that he had discovered a law of nature. Meyer thought it inappropriate to make predictions in this way, as (in the words of Gordin), he wanted to "prove a point about the conjunction of observation and theory." (2012 74).

8.3 How Completeness and Carefulness Related to Identifying Errors

In this section, I explicate how Meyer's valuing of qualitative completeness and carefulness related to using the curve for identifying errors in experimental results. As I noted in chapter 4, Meyer did not just value carefulness in developing the atomic volume curve; there is also evidence for Meyer's valuing of qualitative completeness. At first, I will show how the qualitative completeness of the graph helped to find a suitable vacancy on the graph for indium. I will then show how especially carefulness contributed to Meyer's use of the system to detect errors.

Completeness and the Identification of Errors

Meyer's atomic volume curve mapped the periodic relationship between just two properties: atomic weight and volume. Although Meyer stated that it was best to focus on one property at a time, he also brought several other chemical properties into his graph: volatility, ductility, and electrochemical behaviour. This he did to suggest that a similar periodic relationship may exist with chemical properties other than volumes. In particular,

[70] See especially Meyer (1872, 338).

he suggested that elements with similar properties could be found at “corresponding points” of the curve (1870, 360). As Meyer’s discussion of these properties reveals the valuing of qualitative completeness in his discussion of the atomic volume curve, I here will briefly summarize Meyer’s review of the abovementioned properties.

Meyer’s discussion started with volatility. He suggested that all of the liquid, gaseous and volatile elements would be found in the ascending parts of the curve. Elements that were liquid (presumably in room-temperature) could be located in the sections II and III of the curve, either at the minimum or near it. In contrast, the elements that were hard to melt were to be found from the descending sections of IV, V, and VI, near to minima. Meyer then suggested that light and ductile elements would be situated in the maxima on the descending sections of the curve. In contrast, heavy ductile elements were to be found on the minima of IV, V, and VI, and ascending areas straight after them. Heavy and brittle elements would be located in IV, V, and most likely also in VI, just before the minima of the descending section of the curve. Brittle and non-metallic elements were situated on the ascending parts of the curve that precede maxima. With respect to the electrochemical behaviour, Meyer drew attention to how electropositive elements tended to be on the descending sections of the curve, whereas the electronegative ones were on its rising parts. At the minima and maxima, and shortly after them, we can observe electropositive behaviour of the elements. Before maxima and minima, the behaviour appeared negative (1870, 360-363). Meyer marked all these properties on the curve in writing (see figures 8.1, 8.2, and 8.3 for examples).

As the valuing of completeness involved taking into account several different features on the chemical elements, it speaks of the completeness of the system that Meyer took into consideration the abovementioned properties even though the curve primarily explored the relationship between just two – weight and volume. Furthermore, we may link Meyer’s consideration of metallicity, ductility, and electropositivity – or his valuing of qualitative completeness – and his subsequent process of identifying a more suitable atomic weight for indium.

This link becomes clearer if we consider how Meyer ruled out the second option for indium's atomic weight. When we inspect arsenic and selenium on the atomic volume curve, it becomes apparent that Meyer has written that we should expect finding elements that were "*leicht flüssig u. flüchtig*" (slightly liquid and volatile), "*elektronegativ*" (electronegative) and "*spröde*" (brittle). This indicates that all the elements in that area – arsenic, selenium, and potentially indium – ought to have these properties. As we saw, Meyer suggested that a ductile electropositive metal such as indium would *not* fit alongside volatile, brittle, and electronegative elements. Indium could not belong to that position on the curve because its qualitative properties did not match those of the elements belonging to that area. Thus, we get a sense of how the spread of electropositive and electronegative qualities guided Meyer's shepherding of indium, which suggests that the valuing of qualitative completeness guided Meyer's process of identifying errors in indium's atomic weight determination.

Carefulness and error identification

In what follows, I will argue that apart from the link between qualitative completeness and ruling out options for indium's atomic weight, there is even more distinct link between Meyer's valuing of carefulness and his attempts to identify errors in experimental results. As we saw in section 8.2, Meyer's identification of errors in experimental results was guided by the regularity identified by the periodic systems. Meyer's reliance on the regularity raises the following question: how could Meyer, who emphasised the importance of empiricism, rely on the regularity so much as to use it deem empirical observations flawed? After all, the regularity could not be observed in the same way as brittleness or volatility of elements but emerged from representing various observations on the chemical elements.

The role of carefulness for Meyer helps to answer this question. As noted earlier, I understand carefulness as

> *signalling differences in the quality of observations of the phenomena on the problem area, and prioritizing observations of higher quality over those that are lacking.*

In what follows, I argue that Meyer's valuing of carefulness in constructing the system allowed him to use the system in identifying errors in experimental results. This is because prioritisation of well-established observations then gave rise to a theoretical regularity (or a generalisation) that weighed more than a single observation did on its own. In short, the cumulative effect of individual observations of high quality weighed more than a single empirical result. In order to make this link between carefulness and the use of the periodic system more explicit, I inspect how the different indicators of carefulness found in Meyer's discussion of the periodic system came together in the atomic volume curve. As we saw in chapter 4, I identified three indicators of carefulness, which can be summarized in the following table 8.5.

From the three indicators, the second and third are more relevant for the atomic volume curve, as Meyer's employment of exact atomic weights is difficult to appreciate on graph paper. (This is not to say that Meyer did not try to draw the data points so as to reflect the precision of the atomic weights – as can be seen on the graph, chlorine, Cl=35.5, is drawn between a square of the graph paper. However, the difference between one decimal point and two decimal point precision had only a minor effect on the overall trend of the curve.)

As to the second indicator – making uncertainties explicit – we saw that Meyer drew the less-certain results with a dotted line. Furthermore, from 1872 onwards, he even included gaps in the curve to signal how data did not warrant drawing general conclusions at all. As to the third indicator – unwillingness to combine precise data with unprecise kind – we saw that Meyer excluded several elements from the graph of 1870 (which had altogether 57 elements). Apart from the well-known hydrogen, Meyer excluded yttrium (Y), erbium (Eb), terbium (Tb), cerium (Ce), lanthanum (La), didymium (Di), thorium (Th), uranium (U), and

1. Expressing atomic weights in an exact form.	2. Making uncertainties explicit.	3. Unwillingness to combine precise data with imprecise data.

Table 8.5 The indicators of carefulness

jargonium (Jg) on the grounds that their atomic and equivalent weights were unknown (1870, 357).[71] Thanks to this choice, the system included only those elements whose weights were known with roughly the same level of accuracy. This allowed Meyer to state the generalisation that emerged from the individual data points with about the same level of confidence. Had Meyer included elements that were not equally well known, then there would have been "areas" to the generalisation that would have been less supported than others.

The second and third indicators of carefulness directly influenced Meyer's use of the curve for error identification. In particular, when we consider the second indicator of carefulness, it is notable that Meyer did not suggest an option for indium's weight on the dotted lines of the curve (i.e., sections I, II and VI). This makes sense. In order to stress that the (theoretical) regularity identified by the graph *could* be used as a reliable guide to assess individual empirical results, using the trend of the better-known areas reduced the risk of error in the diagnosis of error.

As to the importance of the third indicator, Meyer's privileging of the elements with well-known atomic weights allowed Meyer to rely on the generalisation that the system put forward. Earlier, I noted that periodicity could not be observed in nature in one glance like brittleness or volatility. Instead, we gain access to it through a representation that brought together several individual empirical results. Having high standards in selecting the empirical data – especially using atomic weights that were determined through multiple methods – could then give rise to a generalisation that could be reliably used to identify errors in individual empirical results. This use of the system required that Meyer traded off quantitative completeness (of including all of the substances that were suspected to be chemical elements) with carefulness in his prioritization of well-founded observations.

[71] The graph included in the 1872 edition of the textbook has 56 elements. Interestingly, the graph that Seubert included in his compilation of periodic systems had no more than 57 elements despite the fact that the book was published no earlier than 1895.

However, the role of carefulness in helping Meyer to identify errors in empirical results extends the assessment of indium's atomic weight. In the next section, I will show how carefulness in selecting observations was also important for Meyer's broader project of establishing the usefulness of theories in chemistry.

8.4 Carefulness and theories

So far, I have demonstrated that Meyer's valuing of carefulness and qualitative completeness helped him to use the atomic volume curve to identify errors in experimental results. In what follows, I will show why carefulness in selecting the observations was important for Meyer's overall project of promoting a greater role for theories in chemistry. In particular, I suggest that carefulness was needed to mitigate the worry that theories were not useful for chemical investigations, and could lead them astray.

We will get a better understanding of how valuing of carefulness relates Meyer's overall project of establishing theoretical chemistry when we look at the textbook where Meyer developed his earliest systems. As noted in chapter 4, the aim of Meyer's textbook was to make a case for the helpfulness of theories in chemistry. However, in 1864, Meyer noted that there was still widespread suspiciousness towards their helpfulness amongst chemists:

> [I] had hoped to prepare the way for the further development of theoretical chemistry … but when, two years later, after having for the third time revised the manuscript, it was handed to the printer … I seriously doubted whether it might prove useful to chemistry or its representatives*. Being fully aware that a work dealing with theoretical matters alone was in direct opposition to the prevailing custom,* which permitted only a few careful speculations and theoretical considerations to be mentioned in connection with the results of experimental investigation, and although no pains had been spared, I yet entertained grave doubts as to whether the result was such that my colleagues would be ready to extend due consideration to one who had thus shown so little regard for the accepted custom. (Meyer 1883 reprinted in Meyer 1888, vii-viii, emphasis added).

Although Rocke (1984) argues that chemists relied on theoretical assumptions in their practice – and Meyer's later statements of 1893 convey a similar message – the above statement from Meyer shows that he initially perceived that speaking in favour of theorising was discouraged.[72] However, in Meyer's later outline of the process of developing the textbook, he suggested that already in 1860s, he had viewed that such scepticism was somewhat misplaced:

> When, in the year 1862, I undertook this work, it was with the desire and hope that its publication might contribute to the removal of the doubts and uncertainties, so frequently expressed at that time, as to the character of the views and theories then contending for supremacy in chemistry. It was my firm conviction that the confusion in the discussions of that time chiefly arose from erroneous views as to the importance of hypotheses and theories, upon which some were inclined to place too high a value, whilst others again did not sufficiently recognise their true importance. *The chief aim of my undertaking was to show that hypotheses and the theories based upon them are necessary aids to chemical investigation, and at the same time to assign to them their true value and place them in a position similar to that which they occupy in theoretical physics*. (Meyer 1883 reprinted in Meyer 1888, vii, emphasis added).

This suggests that Meyer's goal was not to straightforwardly promote theoretical chemistry. Rather, Meyer wanted to argue in favour of the helpfulness of theories *while simultaneously stressing the dangers of unfounded hypothesising*. For this reason, his account should not be seen a defence of a first-principles approach to chemistry. Instead, it should be viewed as an invitation to consider how some theories and hypotheses could helpfully guide chemical investigations. Theories were not an end in themselves, but that they derived their importance from how they could aid (empirical) chemical investigations.

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[72] For an account emphasising research chemists use of atomic theory in 1800-1860, see Rocke (1984). For the theoretical approaches that characterised chemical explanations from the late eighteenth century onwards, see Nye (1991, Ch.3, especially 58-72). For a shorter sketch of the development of theories in chemistry more generally, see Rocke (2018).

When we consider his statements in the original 1864 introduction to the textbook, Meyer suggested that some of the communal scepticism was caused by the premature interest in pushing for theoretical chemistry. Meyer especially brought up the French chemist Claude Louis Berthollet (1748-1822). Although Meyer regarded Berthollet's theoretical chemistry as valuable, he also argued that Berthollet's attempts of establishing theoretical chemistry in 1803 had been done at a time when not enough facts and observations had been gathered. According to Meyer, Berthollet believed that the state of development of chemistry was more advanced than it actually had been. However, in hindsight, this had not been the case, for theoretical chemistry "requires the exact knowledge of an extremely large number of chemical compounds, for without such a knowledge chemistry would soon run the danger of being stranded" (1864 in 1888, xxv). It was only

> When the enormous material … has been arranged and classified by the untiring zeal of the most gifted investigators, then only can we think of laying the foundation of a building, which perhaps in the next century will develop into a theory of chemistry — a theory such as that of light and of electricity, by means of which from given conditions phenomena may be predicted. Chemistry is still far distant from the goal which Berthollet desired to reach; but still much has occurred which indicates that the science will continue to strive successfully to gain this point. (Meyer 1864 in 1888, xxv-xxvi)

Meyer saw that there was no reaching of Berthollet's goal unless theories were founded on observations, and in order to avoid issues invited by unfounded hypothesizing, chemists should not lose sight of their importance. As to reaching the general goal of theoretical chemistry, Meyer instructed that "chemists are compelled to observe phenomena step by step; nevertheless, it is to be expected, that as the principia deduced from such observations become more general, they will more nearly resemble the principles of mechanics. *Observation alone must lead to this stage of perfection*, which may now be regarded as the goal to be attained" (Meyer 1864 in 1888, xx, emphasis added).

Awareness of the background against which Meyer developed his periodic systems

suggests the following role for carefulness in Meyer's broader project. When proposing new theories, there was a heightened need for carefulness in the curation of observations that were used as the basis for theories. This was because the valuing of carefulness helped to mitigate the threat that theories could lead chemical investigations astray. If the theory or the generalisation was not firmly rooted in observations that were deemed reliable, or if the observations were not approximately equally well-founded, it could lead to further undue distrust in the usefulness of theories. Bringing attention to the quality of observations, and *prioritizing* well-established observations allowed Meyer to construct a system that could be relied upon in assessing individual empirical results.

8.4 Conclusion

In chapter 7, we saw that the valuing of completeness supported Mendeleev's predictions of the properties of unknown elements and his corrections of data of little-known elements. In this chapter, I argued that Meyer's valuing of qualitative completeness helped Meyer to identify the best position for indium in the periodic system. Although by doing so, qualitative completeness helped to identify errors in indium's atomic weight determinations, I suggest there is an especially strong connection between Meyer's valuing of carefulness and his use of the graph to identify errors. More specifically, the valuing of carefulness in developing his systematisations facilitated using the periodic regularity as a diagnostic tool to evaluate individual empirical results. As we saw, such a strategy supported Meyer's broader aims concerning the relationship between theories and the empirical practice of chemists.

Chapter 9. The Virtues of the Values Framework

9.1 Introduction

In this thesis, I provided an example of an integrated history and philosophy of science (iHPS) approach to values in science. I did so by applying the framework of values to describe how Newlands, Meyer, and Mendeleev developed their periodic systems of chemical elements. In section 9.2 of this chapter, I summarise the arguments presented in this thesis. Apart from offering a novel interpretation of the early periodic systems of the 1860s, this thesis also supplements the existing accounts on periodic systems by placing the process of developing them at the very centre stage of the discussion. More often, historical and philosophical accounts focus on their reception (Brush 1996; Kaji, Kragh, and Palló 2015) and when they do devote attention to the process of development, the accounts tend to be brief (Rawson 1974; Gordin 2004).

In section 9.3, I show what my take on values and the early periodic systems offers for the existing philosophical literature on values in science. In particular, I argue that an iHPS approach to values can enrich our philosophical accounts as a closer study of concrete historical cases paves the way for more thorough analyses of individual values. Furthermore, the close study of primary historical sources allows us to test the suitability of philosophers' collective categorisations of values. Apart from supplying a more in-depth account of completeness, carefulness, and simplicity, the iHPS take offered here suggested that values influenced specific applications of the systems. I conclude section 9.3 by turning my attention to the more methodological lessons offered by this thesis.

In section 9.4, I demonstrate how this thesis relates to the historical literature on values in science. In particular, a brief comparison with the existing historical accounts of values helps to highlight the limitations of this thesis; in contrast to Graham (1981), Wise et al. (1995) and Stanley (2007), I have mostly focussed on the "internal" role of values in guiding the development of the systems. Extending the investigations to values from the process of development to their reception would offer one plausible way of expanding the scope of this thesis.

9.2 The Framework of Values and the Early Periodic Systems

In chapter 2, we saw that Newlands frequently evoked the "simple relation" in the discussion of his systems of 1863-1872. Although Newlands' systems have received some attention in the literature on the periodic system, Newlands' emphasis on simplicity has not been analysed (van Spronsen 1966; Woods 2015; Gordin 2018). In chapter 3, I sought to show that despite the difficulty of providing a consistent abstract analysis of the "simple relation," examining Newlands' development of the system with a special focus on his invoking of simplicity offers an informative way to analyse his process of constructing the systems. In particular, the focus on values shows historical actors may have elevated different aspects of the valued properties of their arrangements, where at times, they could be in tension.

As we saw, initially, Newlands expressed the view that the source of simplicity was the regular numerical differences among analogous elements. Although Newlands did not evoke "simple relation" when introducing his system of 1865, in chapter 6, I demonstrated how the look of the systems of 1865 and 1866 appeared distinctly simple, where I updated Currie's account of theoretical simplicity to illuminate how Newlands minimised visual features of the system and cut the previous organising parameters guiding his systematisation. I then demonstrated how Newlands emphasised simplicity in the subsequent defences of his system. In particular, his comments implied that the "simple relation" served as evidence in favour of his discovery of a law of nature, or, to use Michael Polanyi's terminology, identification of a "significant pattern" in nature (1958, 37-38). However, as we saw in chapter 6, the differences of Newlands' emphasis on the "simple relation" did not pave the way for a coherent abstract characterisation of simplicity. As such, Newlands' case gives us an example of how we need to adjust our (philosophical) expectations about the coherence of actors' categories.

After detailing Newlands' valuing of simplicity, I set out to show that Meyer especially valued carefulness in developing his systematisations, where I understood carefulness as distinguishing between observations of high quality from those that were lacking in one way or another. Furthermore, I suggested that carefulness also involved prioritising the high-quality observations over inferior ones. As we saw, Meyer excluded chemical

elements which he regarded as more dubious. Especially interesting was that Meyer employed several visual strategies to signify different degrees of certainty in the observations. For starters, he expressed some atomic weights with up to two decimal points to signify the high-quality of atomic weight data. We also saw that Meyer used different kinds of lines for different kinds of data in his atomic volume graphs. As to other values present in his approach, I suggested that Meyer valued qualitative completeness, as he related several qualitative properties in his discussion of the atomic volume graph.

In chapter 8, I argued that Meyer's emphasis on valuing of carefulness had an important consequence for the applications of his system; it rendered the periodic system useful for identifying errors in experimental results. While Meyer discouraged using the system for predicting properties of unknown elements, he allowed using the systems to identify which atomic weights or volumes had been inappropriately determined. Using the system in this way then allowed Meyer to evaluate competing atomic weight determinations.

There are several benefits in taking such an approach to Meyer's systems. For starters, I introduced a new value of carefulness, which may be fruitfully applied to other contexts. Furthermore, the discussion of Meyer's systems is to be welcomed more generally, because by and large, Mendeleev has received more attention in the literature (see also Chang 2012a, 145). For example, Scerri's (2007) account of the periodic system is clearly balanced in favour of Mendeleev. Gordin's comparison of the approaches of Mendeleev and Meyer (2012) offers a welcome closer look at Meyer and highlights how his systems are often taken as the foil to Mendeleev's approach without closer reflections on why Meyer organised the elements the way he did. I suggest that especially the connections I identified between Neumann's Königsberg seminar and Meyer's approach to systematisation offer a helpful addition to the literature on Meyer's periodic systems.

As for Mendeleev, we saw that authors often bring attention to the completeness of his system without explaining how his systems were more complete (Brooks 2002, 129; Scerri 2007, 123; Gordin 2018, 11). Often, they seem to imply that the completeness of the systems had to do with the high number of elements included. However, in chapter 5, I argued that Mendeleev's valuing of completeness did not solely derive from the inclusion

of many chemical elements. Rather, his valuing of completeness also concerned accounting for many of the qualitative similarities of the chemical elements. Although I sought to show that both the systems of Newlands and Meyer were also somewhat complete, in contrast to Newlands, Mendeleev's explanations of how the qualitative properties of elements were distributed on the system were much more thorough. Although Meyer, too, brought together several qualitative features to his system, the comparisons between Mendeleev's and Meyer's systems show that Mendeleev's ambitions for completeness were greater, as he was more permissive in including the more dubious chemical elements to the systematisation.

Applying the framework of values to analyse a fairly thoroughly characterised episode in history of chemistry has helped to bring to the surface some contrasts and similarities between the systems that could have been easily missed with other analytic frameworks.[73] With Mendeleev's system, we saw that it was not more complete than the others just because it incorporated more elements, but because the system brought in a greater variety of qualitative features. With Meyer, commentators often bring attention to how his system was incomplete (Scerri 2007, 93; van Spronsen 1969, 125). However, the framework of values highlights the fact that Meyer had a convincing argument for excluding some elements from his system; he excluded elements because he valued carefulness in the selection of observations that subsequently gave rise to theory. In addition, the comparison of the systems of Meyer and Newlands revealed their differing emphasis on simplicity. Where Newlands saw simplicity as a valuable quality for his systematisation, Meyer discouraged assuming underlying simplicity.

Apart from bringing forth similarities and contrasts which would have been easily missed otherwise, an essential benefit of the framework of values is that it helps to account for the development of the periodic systems in a manner that does not make their organisation appear like a list of isolated technical decisions. In particular, the framework of values highlighted the hidden similarities between many different design choices. For example, it

[73] Existing accounts of the history of the periodic system include at least, Venable 1896; Tilden 1910; Kedrov 1958b; van Spronsen 1969; Cassebaum & Kauffman 1971; Rawson 1974; Smith 1976; Bensaude-Vincent 1986; Giunta 1999; Gordin 2004; Scerri 2007; Gordin 2012; Gordin 2018.

helped to show that Meyer's use of precise atomic weights and the dotted line were not two entirely unrelated strategies for presenting data. Instead, the framework of values revealed that both strategies were motivated by the assessment of the quality of observations. This suggests values can illuminate the reasons, preferences, and motivations that powered the design and development of the systems, thus unifying those choices involved in the organisation of the systems that seemed isolated and not very telling on their own.

I maintain that not only does the framework of values make the systems speak to us, but also amplifies the voice of the chemists who issued them in the first place. We should not take this for granted in our analytic frameworks. For example, in the existing literature on periodic systems, some authors seek to explain the organisation of the early systems by identifying the relevant conditions for what counts as a periodic system and then evaluate the early systems in terms of how well they satisfy those conditions. For example, this appears to be the case in H. Cassebaum and G.B. Kauffman's account (1971), where they list five criteria that a systematisation has to satisfy in order to count as a periodic system. Similarly, Giunta denies that Newlands' tables satisfy the conditions that would allow calling it the periodic *system* of chemical elements (Giunta 1999, 24-30). Such an approach is also at times adopted in van Spronsen's evaluations of early systems, where, for example, he suggests that Newlands' earliest system were "not ideal" and denies that his system of 1864 was a periodic system (van Spronsen 1969, 104-105; 134-135). As Gordin has pointed out, such an approach paves the way to situations where we title one of the six chemists as *the* discoverer on the system based on which criteria we take as the core of the periodic system (Gordin 2012, 61).

Another popular way of enticing the early systems out of their silent shells is to explain their organisation in terms of how well they match with the modern periodic systems. For example, in Scerri's explanation of Meyer's system of 1868, Scerri credited Meyer for "*correctly* placed mercury with cadmium, lead with tin, and thallium with boron" (2007, 98, emphasis added). This comparative way of explaining the early systems is especially prevalent in van Spronsen's account of early periodic systems. For example, when discussing Meyer's system of 1868, van Spronsen stated that "A study of the system (…)

shows that Meyer succeeded in including all the elements in main and sub-groups and thus in forming a periodic system" (van Spronsen 1969, 127). (Note that the terminology of main and sub-groups is more familiar to us from explanations of the layout of the modern periodic systems.) In a similar vein, van Spronsen stated that Mendeleev's first system

> *encompasses almost all the facets of a true periodic system of elements and should therefore be seen as the culmination of the period of discovery.* Special mention may be made of certain aspects: the division into main and sub-groups … the classification of transition metals, and the reversal of tellurium-iodine (van Spronsen 1969, 134-135, emphasis added)

Although no-one would deny that explaining the early periodic systems in relation to the modern ones can help to understand how the first systems gradually transformed into the familiar shape we encounter on t-shirts, flasks, mugs, books, and walls of classrooms, it comes as no surprise that explaining their development primarily in the terms of the modern systems is problematic. Firstly, such assessment risks that the authors see merit in the early systems only if they are similar to the modern ones. This issue is especially pressing with the judgement that Meyer's systems were incomplete and therefore less praise-worthy than those of Mendeleev's. Applying the framework of values to analyse Meyer's approach highlighted that Meyer had a convincing reason for excluding many chemical elements. However, his argument would have remained hidden if we solely explained Meyer's system in terms of how it relates to the modern ones. Thus, the most serious issue with analysing early periodic systems in relation to the modern ones is that such explanations offer a limited kind of understanding which risks concealing the reasons that the chemists had for their organisations. Furthermore, even if we granted that such comparisons would be the most fruitful way to explain the organisation of the early systems, we should remain aware that our modern periodic system is flexible, mouldable, and shape-shifting, and the debates concerning the "optimal" periodic system are still ongoing (Scerri 2009).

I suggest that there is the following benefit in using the framework of values: it lets us explain the organisations of the early periodic systems in a manner that enables placing

the chemists' arguments and reasons at the very forefront. In contrast, too firm a reliance with comparisons with the modern systems risk importing a triumphalist "winner's history" and giving up one of our motivations for pursuing historical research: the resurrection of objects, products, practices, and representations that have been downplayed, marginalized, or forgotten (Chang 2009, 252-253; see also Friedman 2018, 5-15). As we saw, Meyer's exclusion of many elements from his system was something that rendered his systems different from those of the other chemists. However, investigating Meyer's process of developing his systems with the framework of values revealed that he had a good argument for doing so.

This is not to say that the framework of values is free from risks; there remains the possibility of distorting actors' arguments with overt reliance on our current understandings of values. (As we saw in chapter 6, this was the risk with using 'accuracy' as the main category to explain Meyer's approach to systematisation). I suggested that this issue can be mitigated with greater reflexiveness and transparency over the choice of analytic categories. Although the framework of values is not entirely free from risk, it is worth stressing that it still does not run quite as great a risk of a "winner's history" as analyses that depend on comparisons with the modern tables. This is because from the get-go, values are taken as characteristics and qualities deemed worthy, positive or desirable – an understanding broad enough to accommodate differences in different historical situations. Thus, at least in principle, the framework enables to explain the chemists' processes of developing their periodic systems in terms of their own. This is less so the case when authors analyse the organisation of the early periodic systems in terms of the modern ones.

Aside from showing that the framework of values helped to unify isolated technical choices and to explain the early systems in terms of the chemists, this thesis has also introduced some findings beneficial both for philosophical and historical literature on values in science. In the following section (9.3) I show how my more historically oriented take on values benefits philosophical literature on values, where I argue that a more in-depth historical investigation of values can offer new avenues for research in values, as called for by Elliott and McKaughan (2009). In particular, an iHPS take on values offers good

material for making more thorough analyses of individual values, serves to test accounts of general categories for values, and helps to tease out the relationship between values and the further applications of scientific representations.

9.3 Philosophical literature on values in science

The idea that philosophers of science should primarily focus on the narrowly-construed context of justification at the expense of other contexts has now given way to philosophical interest in many different contexts of scientific practice (Hesse 1966; Nickles 1980; Anderson 2006; Schickore and Steinle 2006; for the context of pursuit, see Šešelja, Kosolosky, and Straße 2012; Šešelja and Straßer, 2014; Nyrup 2015). The philosophical interest in values in the contexts other than justification is poignantly illustrated in Kevin Elliott's *Tapestry of Values: An Introduction to Values in Science* (2017), where Elliott identifies four ways in which values may impact scientists' decision-making: through influencing the choice of topics (chapter 2); how to study a topic (chapter 3); the aims of scientific inquiry (chapter 4); and responses to scientific uncertainty (chapter 5). Although Elliott rarely explicitly refers to processes of development, it is evident that all the aforementioned ways that values influence science also apply to the processes of developing theories, explanations, models and other representations in science.

Elliott's most explicit discussion of how values influence the process of developing representations comes from his treatment of models. In particular, Elliott evokes Kirstin Intemann's account of the influence of (nonepistemic) values on climate models, where Intemann argues that their influence is legitimate insofar as nonepistemic values further epistemic and social aims of research which have been democratically endorsed (Intemann 2015, 219). With a similar focus on the context of development, Nancy Tuana has argued that we need to train students in the ethical decisions involved in the development of models (Schienke et al. 2011; Elliott 2017, 78). Both investigations illuminate how ethical values may legitimately shape the development of models in science.

In a similar vein, Heather Douglas' outline of the state of literature in values shows that although authors only rarely explicitly bring up the context of development, many of their

arguments and observations could plausibly be extended there (Douglas 2016). Apart from the role of values in the construction of models, values influence the methodological choices of scientists where Douglas brings up Steel and Whyte's (2012) treatment of how moral and ethical considerations can override epistemic considerations in methodological choices (see Douglas 2016, 623-25 for further examples). Furthermore, the very choice of language and terminology is value-laden, which also gives us an example of the role of values in the development of scientific representations (see Dupré 2007; Alexandrova 2015, see also Alexandrova 2016). To broaden our scope outside Elliott's and Douglas' overviews of the state of the literature on values in science, we should also mention David Ludwig's argument that values influence the very ontological choices made by scientists, which, too, counts as the influence of values in the development of theories (Ludwig 2015).

All of the above accounts of values suggest that the context of development has not been devoid of attention. However, they also reveal that authors mostly bring up examples from recent science. In Elliott's overview, the more historical examples come from Soviet science (2017, 1-5) and from philosopher Janet Kourany's historical investigations to the justifications of women's inferiority in science (see Kourany 2010; Elliott 2017, 21-22). The concern with examples from more recent science also characterises Douglas' overview. Although some of the authors evoked by Douglas do give more historical examples of values in science (e.g., early 20th century primatology in Haraway 1989; for more brief examples, see Newton's theology in Rooney 1992; Kuhn 1977; McMullin 1982) the majority of the examples and case studies arise from the more recent scientific practice.

One of the (implicit) aims of this thesis has been to show that our philosophical accounts of values could benefit from much closer engagement with the primary historical (and secondary) sources. In particular, this thesis demonstrates that a closer investigation of primary historical sources can provide a more in-depth account of values. In particular, I have shown that it can enhance our understanding of specific individual values. For example, we saw that a closer analysis of the completeness of Mendeleev's system paved the way for a more substantial philosophical analysis of the value, which may then be adapted to other contexts later. In particular, investigating the completeness of

Mendeleev's system demonstrated that we need not automatically discard completeness as an unattainable ideal for science. Insofar as we have an idea of the boundaries for a particular task that the investigators have taken up, completeness becomes something that investigators can achieve and something that we can analyse. We also saw that completeness could come in degrees; one system can be more complete in accounting for the physical properties, and another for the chemical ones. The definition that I put forward is flexible enough to grant the completeness of one system in isolation while also allowing us to articulate how some systems might be more complete than others.

Newlands "simple relation" then suggested that it would be helpful to supplement our existing philosophical literature on simplicity beyond accounts of parsimony. Furthermore, Newlands' case demonstrated that sometimes our philosophical ideals about attaining consistent analytic categories should be relaxed, if the historical evidence suggests that the historical actors were not consistent in how they used their terms.

With Meyer, we saw that although Meyer's valuing of carefulness in curating the chemical elements to the system attached specifically to his assessment of *observations,* it also played into his broader views on the relationship among observation, experiment, and theory. Meyer's example suggests that when we introduce philosophical analyses of accuracy, we need to also take into account why some investigators have found the idea of accuracy as closeness to truth problematic. The closer study of Meyer's approach to systematisation (and his views on chemistry more broadly) made it clear that the existing philosophical literature on accuracy needs to take into account that accuracy as closeness to truth is not the only way of conceiving accuracy.

Apart from enhancing our accounts of individual values, a closer study of the historical primary sources also helps to test philosophers' more collective categorisations of values. As emphasised in chapter 1 of this thesis, our traditional division between epistemic and nonepistemic values cannot always be maintained. In particular, we saw that the way authors often define 'epistemic' and 'pragmatic' conceals the possibility that some individual value could simultaneously be characterised as epistemic or pragmatic (or as something else). It seems like the drive for clarity and clear-cut distinctions can sometimes

lead to an underappreciation of the ambiguity of values. As we saw, looking at values in their local historical context shows how a single value might simultaneously be both categorised as pragmatic and epistemic. This was especially the case with completeness, which had a distinctly pragmatic function in the sense that Mendeleev regarded it as pedagogically useful to include all of the elements to the system, but also an epistemic function in a narrower sense.

The ambiguity of completeness demonstrates how even those values which we would expect to serve as the clearest examples of distinctly *epistemic* values can also act also as examples of nonepistemic, noncognitive, or contextual values (see also Rooney 1992; Rooney 2017). Even when we are examining the most persistent scientific representations that have emerged from the physical sciences, it is far from straightforward that the values guiding their development would be solely truth-conducive or oriented towards empirical adequacy. Although such suggestions on ambiguities of values are not new (Rooney 1992; Rooney 2017), authors still largely rely on the epistemic/nonepistemic (or cognitive/noncognitive) distinction to show the legitimacy of some noncognitive values even in the light of the broader recognition of the difficulties of maintaining the distinction (Steel 2010; Douglas 2013; Intemann 2015; Elliott 2017). If we recognise the importance of both respecting the ambiguity of the values, and the helpfulness of having clear distinctions, I suggest it may be in some cases helpful to replace our broader categories of values with the language of different functions of individual values. I advocate such phrasing because it draws attention to the possibility that an individual value could plausibly be categorised in several different classes rather than just one.

Apart from paving the way for a deeper understanding of individual values, and revealing the issues with broader categorisations of values, the close examination of primary sources demonstrates how emphasis on some values during the development of the systems influenced their further applications. In particular, we saw that the valuing of completeness supported using the systems to make different kinds of predictive statements. In chapter 7, we saw that especially the schematic row of oxides guided Mendeleev's corrections of the properties of little-known elements, and his detailed predictions of the properties of unknown elements. This implies that Mendeleev's

predictions of 1871 depended on Mendeleev's inclusion of different findings on the qualitative properties of the chemical elements into his systematisation. With the exception of Chris Campbell (2017, 213-220) the close connection between accommodation of data and the use of the system for making predictions has not been emphasised enough in the literature on the epistemic merits of prediction of new phenomena versus accommodation of known data (Lipton 1990; Barnes 2008; Schindler 2014).

With Meyer, we saw how his emphasis on carefulness in choosing which observations to include in his system rendered it useful for identifying errors in experimental results. Because the theory was established in observations that Meyer had deemed satisfactory, he could then use the generalisation to identify a potential error in observations. As I noted in chapter 8, Meyer's position is similar to constructive empiricism, where the literature on empirical adequacy could benefit from drawing from his case (van Fraassen 1980).[74]

It remains to be seen whether the individual values of carefulness, completeness, and non-parsimonious simplicity can be usefully applied to other contexts. One plausible way of extending this study is to start testing the generality or the scope of the values in the manner suggested by Scholl (2018), Sauer and Scholl (2016), and Chang (2012b).

Apart from providing a more thorough account of individual values and suggesting that values also support specific uses of the systems, this thesis has also put forward some findings concerning the methodology of iHPS investigations of values in science. For starters**,** investigating values in the context of development is easier if there are several versions of the representation to contrast with one another. As we saw, this thesis focussed primarily on the systems of Newlands, Meyer, and Mendeleev, leaving out a closer examination of the systems of Odling, Chancourtois, Baumhauer, and Hinrichs. The

[74] Furthermore, it is surprising that the prediction versus accommodation debate has not acknowledged Meyer's argument on why the periodic systems ought not to be used for making predictions of unknown chemical elements. As we saw, Meyer argued that as many theories had previously turned out to be empty speculations, it was important not to overpromise what theories could deliver. To appropriate Popper's analogy, refraining from "sticking one's neck out" could be strategically wise against the background of several necks having been chopped off.

main reason why this thesis focussed on the systems of the three abovementioned chemists is that they left a rich trail of different versions of their systems. The availability of several different versions allows us to observe the changes and alterations that the investigators made to their works. Tracking the contrasts and similarities in this way offers invaluable information about which values guided their development in practice.[75]

In cases where we do not have a trail of different versions to contrast, we may very well find statements where the investigators emphasise the importance of some qualities or characteristics. However, it would remain more difficult to maintain that the investigator valued those qualities in practice if we cannot track whether they elevated those qualities over a longer period of time. In short, we do not have a way of telling whether their reference to virtuous qualities is a rhetorical manoeuvre, or whether they viewed those qualities as important enough to guide their process of development. Having said that, with "one-off" representations we should not rule out the possibility that some values have been central in the process of moulding them. Although this information might be lost from us due to the disappearance of preliminary versions, we should not deny that values guided the development of those representations. Instead, I suggest we remain agnostic about which values played a part in their processes of development.

Another important methodological lesson concerns the choice of categories used for analysing the historical case. In chapter 6 I argued that our abstract characterisations of values need to elucidate concrete instances that indicate the valuing of some quality. In particular, I argued that we should utilise the more general philosophical accounts of values (formulated without special consideration of the systematisation of the chemical elements) only insofar as they help to elucidate values in their local context. As we saw, some categories do that better than others. I illustrated this by demonstrating how accuracy, sharpness, and precision did not quite capture the distinctness of Meyer's

[75] I should note that Odling published a system of natural groups (1857), and another that is recognized as one of the early periodic systems (1864), which he included only in the fourth edition of his textbook. However, unlike Newlands, Meyer, and Mendeleev, Odling did not emphasize a specific quality for his systems explicitly. Although he discussed the importance of considering the totality of chemical characteristics, we may investigate whether he valued completeness (1857, 424). I should highlight that there is less textual evidence in favour of such analysis than with Mendeleev.

approach to systematisation, where especially using the realist and veritist conception of accuracy would have distorted Meyer's philosophical views. With completeness we saw that although Chang's definition of completeness (2012a) was a good starting-point for analysing Mendeleev's approach, the specific (local) requirements arising from systematisation called for modifying Chang's definition somewhat. With simplicity, the majority of the existing accounts concern parsimony, which is a bad fit with Newlands' rhetoric concerning the "simple relation."

As such, chapters 3-6 suggested that the priority for a iHPS take on values is to elucidate how the values appeared in that particular local context. While we may of course explore how general some values are (for a good example on parsimony, see Sober 2015; for generalization, see Sauer and Scholl 2016; Scholl 2018), we should be as interested in finding out how specific values appeared in different local contexts beyond seeking to establish how general some values are. In order to achieve a fuller articulation of values in local contexts, we can utilise the resources available in the philosophical literature on values in science. Although Newlands and Mendeleev explicitly brought up the importance of "simple relation" and *polnost'*, and Meyer more implicitly carefulness, we saw that giving a more thorough interpretation of how they conceived such qualities called for extra resources. Crucially, I utilised those philosophical resources both when I put forward my *positive argument* in favour of some interpretation of values (e.g. the helpfulness of Chang's completeness and Currie's theoretical simplicity) and for the *negative argument* against some interpretation (e.g. the unsuitability of veritist notion of accuracy, as seen in Pettigrew 2016). This is not to say that Chang's and Currie's accounts are somehow more generally helpful or useful than that of Pettigrew, but that in making sense of values in this specific historical context, some are more helpful than others. In short, this example demonstrates the helpfulness of going back-and-forth from more abstract philosophical characterisations and the concrete instances of valuing found in the historical sources.

9.4 Historical Approaches to Values in Science

As I noted in the prologue to this thesis, the framework of values have been utilised by historians and philosophers alike, which renders it fruitful for (i)HPS investigations. In this section, I show that contrasting the project of this thesis with the more historically oriented

accounts helps to demonstrate the limitations of this thesis; instead of focussing on the relationship between societal norms and the values guiding the problem-solving activities of the chemists, I have firmly kept the focus on values in the systematisation of the elements.

Most of the historical accounts on values share the interest in examining how values that guide the "internal" workings of science are connected to political, theological, gendered, and cultural values more broadly.[76] Graham pioneered the approach of detailing the role of political values in science (see Graham 1977; Graham 1981). Similarly, the essays in Wise's *Values of Precision* make the case that values that we are inclined to associate with the inner workings of science also have distinct social and political functions, as controlling domains of nature through precision measurement paved the way to control in political and economic domains (Wise 1995, 5).

Most recently, Stanley has investigated the connections (and disconnections) between the astrophysicist A.S. Eddington's Quakerism and his science (Stanley 2007). Stanley shunned abstract categorisations of "science" and "religion" to analyse the connections between Eddington's Quakerism and his contributions to astrophysics because such categories easily lead to "overbroad, Zeitgeist-like claims" (2007, 5-6). Instead, Stanley proposed the framework of valence values which mark similarities between historical actors' work in the scientific realm and their connections to other domains of their lives. In Stanley's words, "[n]ot all electrons help with bonding, and not all values link science to other categories. In chemical terms, some values have greater valency (that is, tendency and ability to interact). Here I am interested in exactly those valence values that *do* move between the scientific and the cultural" (2007, 6). For Stanley, using the "value-driven methodology" in this way exposes the connections between "very technical aspects of science and very rich aspects of culture" (2007, 240).

[76] Here, I am using the term "internal" in the sense promoted by Shapere (1986, 1-9). For a discussion on the helpfulness (or unhelpfulness) of the internal-external distinction, see (Kuhn 1977, 105-126; Shapin 1992; Schaffer and Shapin 1985, xiv-xvii; Chang 2016)

These abovementioned approaches to values help to highlight the limitations of the account I have put forward in this thesis. Instead of examining the links between the societal context in which the chemists operated and the values guiding the development of the periodic systems, I have mostly focussed on values in the more "internal" sense of them guiding the problem-solving activities in chemistry. The fact that the scope of this thesis has been set in this way should not be taken as a statement against the importance of tracking continuities and discontinuities between values in different domains. I have not attempted to provide such an analysis because of the practical challenge of offering both a detailed account of values in the processes of developing the systems *and* their broader societal role in three different national contexts.

Despite this limitation, I have laid the groundwork for conducting such a study on the relationship between values in "internal" and "external" realms. One option would be to track the continuities between values emphasised by the chemists during the development of the systems and their later reception. The recent accounts on the reception of the systems give good grounds for conducting such a study (Brush 1996; Kaji, Kragh, and Palló 2015). However, before setting out to conduct such investigations, it is important to adjust our expectations on what we might find. In particular, we should be careful not to over-emphasise the connection between values in the context of developing the periodic systems and values in their acceptance or rejection. Such caution is required, because there may be reasons other than values that influence the acceptance or rejection of the system. Instead of *only* looking for connections between particular values of the systems and their broader acceptance, we should also turn attention to applications and the acceptance of the systems. Notably, both Meyer and Mendeleev articulated specific applications for their arrangements, whereas Newlands' list of uses of his system were distinctly *post hoc,* which may play a part in the reception of the systems.

9.4 Conclusion

Mapping out the development of the early periodic systems of Newlands, Meyer, and Mendeleev with the help of the framework of values offers us a number of benefits. The framework allowed to extract contrasts and similarities between the systems that would have gone easily missing without its aid. Values also revealed the underlying themes of

the individual design-choices involved in the construction of the systems. Furthermore, the framework made the systems speak in a manner that placed the decisions of the chemists involved in their development at the forefront. These findings suggest that applying the values framework to the *development* of scientific representations can be fruitfully exported to other historical contexts, and that doing so would teach us of the many roles of values in science.

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